



PHILOSOPHICAL
TRANSACTIONS,
GIVING SOME
ACCOUNT

OF THE
Present Undertakings, Studies, *and* Labours,
OF THE
INGENIOUS,
IN MANY
Considerable Parts of the WORLD.

VOL. LXII.

L O N D O N :

Printed for LOCKYER DAVIS, in *Holbourn*,
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M.DCC.LXXII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it had been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings,

reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

At a C O U N C I L, January 28, 1773.

Resolved, That after Volume LXII. the *Philosophical Transactions* be published twice in a year; the first publication to be of the months of November and December of the preceding year, and January and February of the current year, as soon as may be after February, under the name of the "first part" of the volume: and the second publication to be of the remaining months unto the recess of the Society, as soon as may be after the recess, under the name of the "second part" of the volume.

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PHILOSOPHICAL
TRANSACTIONS.

- I. *A Letter from James Badenach, M. D. to Mathew Maty, M. D. Sec. R. S. containing a technical Description of an uncommon Bird from Malacca.*

S I R,

Read Jan. 9, 1771. **I** Here present you a curious and uncommon species of bird, which I met with at Malacca in August 1770. The male, female, and two young ones, were purchased at that place from the natives, but died soon afterwards on board

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board of ship, in the passage from that port to China. The character and history of this bird, as they then occurred to me, and were immediately noted down in my journal, in the manner of the great Linnaeus, are as follows.

T A B. I.

Mas. Magnitudo perdricis vulgaris, corpus supra virefcens, subtus nigricans, remiges primores griseæ, cauda brevis rotundata, apice nigro. Frons calva, crista coccinea ex occipite orta, è 15 circiter plumulis sesquipollicaribus, respectu capitis, suberecta, divaricata. Rostrum convexum breve; mandibula superior nigra, supra inferiorem, margine rubro cerato, imbricata; nares oblongæ, orbitæ rubræ, oculi purpurei; ad basin rostri mystaces aliquot albescentes. Femora seminuda, tibiæ longæ, rubræ, graciles. Pedes tetradactyli, fissi incarnati, subnodosi, digitus posticus reliquis crassior, brevior, truncatusque.

Fœmina. Mare paulo minor, crista nulla, remiges primores & tectrices alarum rufo ferrugineæ.

Fœmina. Mare paulo minor, crista nulla, remiges primores & tectrices alarum rufo ferrugineæ.

Pulli. Pulli lanuginosi atrî, aqua delectantur.

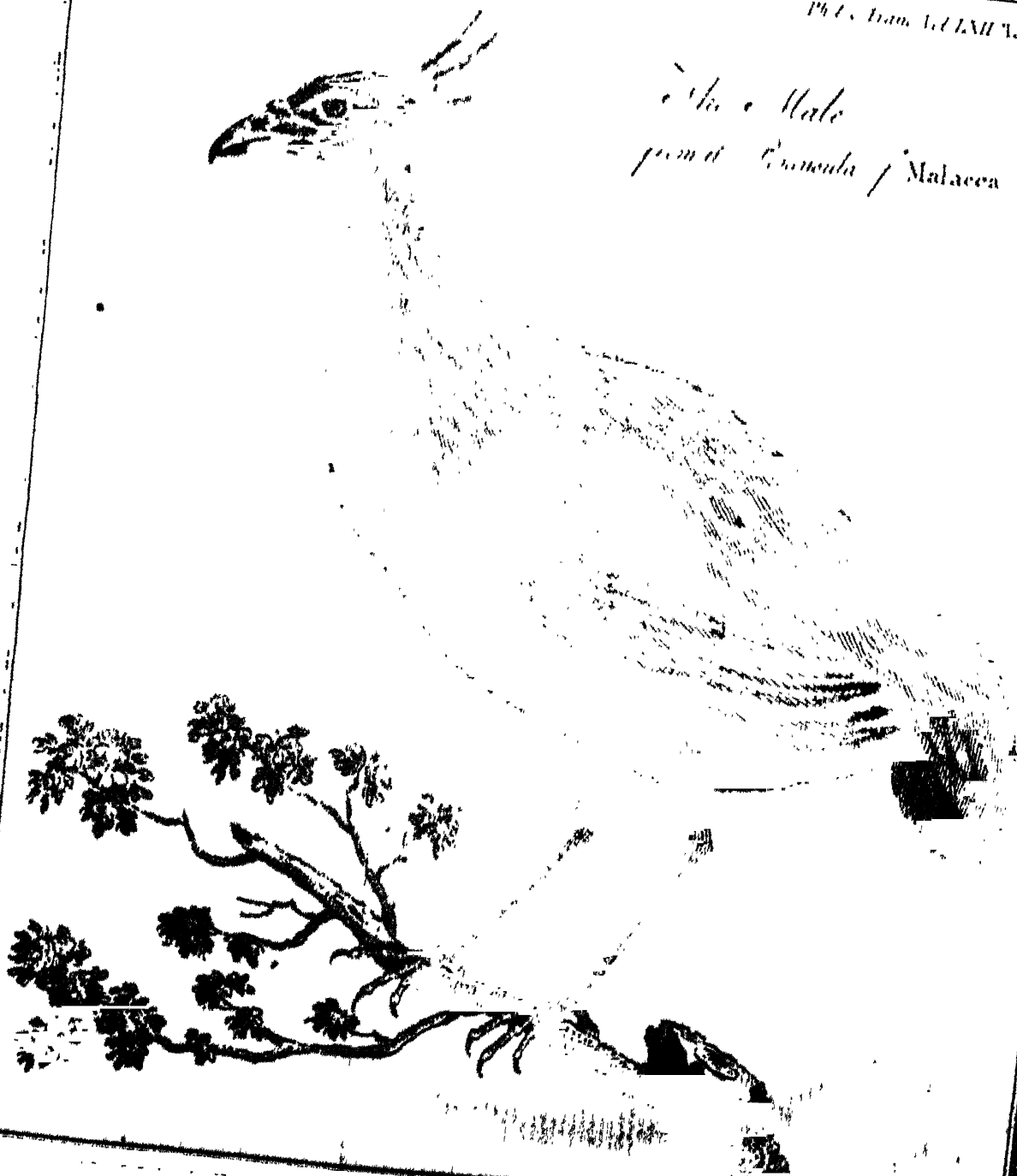
Vox. Mas & fœmina voce sonora frequenter sibilant.

Nidus inter gramina, arundinesque.

Victus. Oryza, pane in aqua missa vescitur.

Phl. from 1st LAM Tab I p 2

Sten. Male
from a *Comenda* / Malacca



Shirley Temple

What genus this bird is properly to be referred to, I shall not pretend to determine; but if you think this, though but imperfect account, worth the communicating to your Society, you have my leave; and I am, with respect,

S I R,

Your most obedient,

and most humble servant,

London,
October 13, 1771.

James Badenach, M. D.

II. *Investigation of the specific Characters which distinguish the Rabbit from the Hare: In a Letter to Samuel Wegg, Esq; T. and Vice-President of the R. S. from the Honourable Daines Barrington, V. P. R. S.*

November 24, 1771

DEAR SIR,

Read Jan. 16. 1772. **I** CALLED lately at your house, to measure some parts of the quadruped which you have received by the last ship from Hudson's Bay, and am now convinced that it should be rather considered as a Hare than as a Rabbit, which latter name it hath obtained in that part of the world.

You will find indeed, from what I am going to state, that it is not very easy to settle a specific, and at the same time scientific difference, between these two animals, even when the greatest authorities in Natural History are consulted.

Ray [a] makes the distinction between the Hare and the Rabbit to consist in the smaller size of the latter, its property of burrowing, and the greater whiteness of the flesh when dressed: he chiefly relies, however, on the one being larger than the other;

[a] Syn. Quadr. Art. LEpus.

as this is the most material circumstance in which they are supposed by him to vary, whether exterior or interior.

Though bulk is undoubtedly a very proper circumstance to be attended to in the description of an animal; yet recourse should never be had to it in establishing a specific difference, except it is the only criterion which can be fixed upon, and the disproportion in point of size is very great.

An Hare, however, does not exceed a Rabbit so much in bulk as a Patagonian does a Laplander, or a mast does a lap-dog, which yet are not to be considered as differing in species.

Besides this, age, climate, and food, as well as other circumstances, often occasion great distinction between animals of the same species, in point of bulk.

The Hare (for example) which is found in most parts of North America, is a third less than the European Hare, and consequently is scarcely larger than our Rabbit [b].

The next criterion which Ray fixes upon to distinguish the Rabbit from the Hare, is that the latter burrows in the ground; this, however, only holds with regard to the warden Rabbit, for those which are called hedge Rabbit, seldom burrow, and many of them sit in forms as Hares do,

[b] Mons. de Buffon is misinformed, when he asserts, on the contrary, that the American Hare is larger than that of Europe, (Hist. N. t. VI. p. 246) if I can depend upon the accounts I have received from those who have long resided in America, as well as some stuffed specimens which I have examined. See also Mr. Pennant's Syn. of Quadr. p. 249.

The

The third and last is, that the flesh of the Rabbit is more white when dressed; which distinction is always to be found between the European Hare and Rabbit, but it does not often happen that one can dress the flesh of an animal which comes from another part of the globe; it is therefore a criterion we can seldom have recourse to.

Linnaeus, thus describes the Rabbit in his *Fauna Suecica*. (Art. *LEPUS*).

Lepus Caniculus, cauda abbreviata, *Auriculis Nudatis*.

Lepus cauda brevissima, pupillis rubris.

With regard to the first circumstance of the Cauda abbreviata, he equally applies it to the Hare in his *Systema Naturæ*, published in 1766, and drops the Cauda brevissima of the *Fauna Suecica*; where in propriety the Rabbit should not have found a place, as it is not indigenous in Sweden, the climate being too cold for it.

Linnaeus therefore could only have described from a tame Rabbit, which I suppose had balder Ears by some accident than common, as his next criterion is *Auriculis Nudatis*.

I have examined lately a great number of Rabbits, and do not find that their ears are balder than those of a Hare: this second circumstance therefore establishes no specific difference.

From the third and last particular which this great Naturalist relies upon, I am also convinced that the specimen before him was not only a tame Rabbit, but that its fur was either white or caroty, because Rabbits of these colours only have red pupils [c].

[c] I have examined a great number of Rabbits thus coloured, which commonly have red pupils, though I have seen some
I find

I find accordingly, that Linnæus hath omitted the *pupillis rubris*, as applied to the Rabbit, in the twelfth edition of his *Systema Naturæ*; but adds another distinction, which will be found equally to fail.

He there says, that the ears of a Rabbit are shorter than the head; whereas those of a Hare are longer: which is a just observation, when the warren Rabbit is examined; but the tame Rabbit (and particularly those which are white or carroty) have ears that are considerably longer than their head.

This circumstance, therefore, establishes no more a specific difference between the Rabbit and the Hare, than the greater length of the ears of a dog would, which in some varieties of that animal are known to be excessively long.

Mons. de Buffon, in his description of the Hare and Rabbit, agrees with Ray that there is nothing either exterior or interior which seems to constitute a specific difference, though he endeavours to establish an incontestable proof that they are really distinct.

He informs us, that he had tried to procure a breed between Rabbits and Hares, but never could succeed in the experiment.

This most ingenious and able writer does not state, however, at what ages the Hares or Rabbits were thus confined, which is known to be a most material

with black: the grey Rabbit however never hath eyes of a red colour. When the white Rabbits are very young, their eyes are often like a ferret's; but when they are grown to their full size, the pupils are generally quite red.

circumstance, by those who have raised male Canary birds [d].

Mons. de Buffon's expression is, "J'ai fait *élever* des hases avec des lapins," which at first seems to imply that he had reared them from their earliest infancy.

Upon consulting however the dictionary of *Tre-voux*, the compilers inform us the word *Élever* [e] often signifies the feeding and keeping an animal, without respect to its age; and they cite its being applied to elephants in Europe, which it is believed never bred in that quarter of the globe.

But the best expounder of the sense in which an author uses a word is in other parts of the same work.

In the fifth Vol. of his *Natural History*, p. 210. Mons. de Buffon gives an account of his making the same sort of experiment between the Wolf and a Dog, in the following words:

"J'ai fait *élever* une louve prise dans les bois, de deux ou trois mois."

In this passage, the word is applied to a wolf, of three months old, and to shew that Mons. de Buffon did not think the age at which the animal is confined to be material in such an experiment, he immediately afterwards states, that he caught some

[d] Birds which differ specifically scarcely ever breed except both are taken early from the nest, and particularly the hen; I have procured a breed from two robins in a cage the present year by attending to this circumstance, and I believe I could equally succeed with almost any other kind of birds, as when they are thus reared, they have not the least awe of man.

[e] "*Élever* signifie, *Nourrir* aussi, soit plante, soit animal, & en avoir soin."

"On a de la peine à *élever* des éléphants en Europe."

foxes

foxes in snares (which were probably therefore full grown), and kept them a considerable time with dogs of different sexes.

After this, he says [e], it is evident from these experiments, that wolves, foxes, and dogs are specifically different, without distinguishing between the foxes being full grown when caught, and the wolf which was only three months old.

But the decisive argument against Mons. de Buffon's experiment not being satisfactory, is to be found in Mr. Pennant's Synopsis of quadrupeds, p. 144: where he informs us, that a breed was actually procured between a dog and a wolf at Mr. Brooks's (animal merchant) in Holborn.

Mons. de Buffon also supposes that the Rabbit is much more sagacious than the Hare, because, both having equal powers of burrowing, the one thus secures himself from most enemies, whilst the other, by not taking the same precaution, continues liable to their attacks.

There are, however, several causes for the Rabbit's burrowing, and the Hare's neglecting to do so.

In the first place, the fore-legs of a Rabbit are shorter in proportion to its hind legs, and at the same time much stronger; the claws are also longer and sharper, resembling much those of a mole.

I have before observed that the Rabbits, which the sportsmen call Hedge Rabbits, seldom burrow; and they neglect taking this trouble, for the same reason that induces the Hare to trust to her form, be-

[e] Hist. Nat. T. v. p. 213.

cause they have an opportunity of selecting a proper place for their concealment.

The ground, however, in a warren, is eaten so very bare by Rabbits, that it is impossible for them to hide themselves if they make a form in any part of it, and they therefore very judiciously choose to burrow under ground.

Another reason, perhaps, for the Rabbit's burrowing arises from the animal's being not only born, but continuing the first six weeks of its life, under ground; they therefore only practise what they have seen and learned in their earliest infancy, as birds from the same circumstance always build their nest in the same form, and with the same materials.

I therefore cannot allow entirely of the distinction arising from the superior sagacity of the Rabbit, because it burrows; and *Monf. de Buffon* himself informs us, that tame Rabbits turned into a warren do not burrow for many generations [f].

Having thus endeavoured to shew that no proper criteria have hitherto been fixed upon to distinguish the Rabbit from the Hare, I shall now venture to suggest the two following, which, I flatter myself, will be found less liable to the same exceptions.

If the hind legs of an *European Hare* are measured from the uppermost joint to the toe, the number of inches will turn out to be just half of the length of the back, from the rump to the mouth (the tail not being included).

The hind legs of the Rabbit being measured in the same manner, and compared with the back are not much more than one third; from which

[f] *Hist. Nat. T. 5. p. 306.*

it seems not unfair to consider any animal of the Hare genus, (whose legs thus measured are less than the half of the distance from the rump to the mouth) as a Rabbit, and on the contrary when they are either one half, or more, as a Hare.

If the fore and hind legs of a Rabbit and Hare are also respectively compared, it will be found that the fore legs of the former are proportionally more short, than those of a Hare.

By both these criteria the quadruped from Hudson's Bay must rather be considered as a Hare, than a Rabbit (as it is called in that part of the world), according to the admeasurements subjoined, which include the respective proportions also of the Alpine Hare [b].

	Fore Leg. Inches.	Hind Leg. Inches.	Back and Head. Inches.
Rabbit	4½	6½	16½
Hare	7½	11	22
Hudson's Bay Quadruped	6⅞	10½	18
Alpine Hare	6¾	10⅞	22
	From the uppermost joint to the toe.	From the uppermost joint to the toe.	

From the proportion of these parts, in the Hudson's Bay quadruped, according to this table, I flatter myself, that it may with greater propriety be classed as belonging to the Hare species, than by any other marks of a specific difference which have been hitherto relied upon.

[b] This species of Hare is found in the Highlands of Scotland, whence I received a specimen, which I had the honour of presenting to the Museum of the Royal Society.

I do not mean, however, to assert from this, that a Hare and Rabbit are certainly of a distinct species; as this can only be settled by failing to procure a mixed breed between the animals after repeated experiments, and under proper circumstances.

I shall now add, that the Hudson's Bay animal also approaches nearer to the Hare than the Rabbit, by the fore legs being much more slender in proportion to the hind legs than those of a Rabbit are; and that the claws are also shorter. As the animal likewise happened to die on the 22d of this month, I boiled the flesh, by your permission, which was as brown as that of the European Hare; and consequently it is to be classed as of that species, according to Ray's third criterion.

But the most curious particular in this quadruped is the white winter coat, which covered, at its death, the greatest part of the animal.

This refutes at once the notion, that animals in the more Northern countries become white by the intense cold of the climate, because this quadruped arrived in England about the time that the change should have begun if it had continued in Hudson's Bay.

As the animal was born, however, in a country where snow covers the ground during the whole winter, it is providential that the formation of its parts and juices should be such as should periodically occasion such a change in the fur; and perhaps, it is the only quadruped which ever was brought from a climate of such rigour, to a more temperate one, before the alteration of the colour in its hair took place.

By

By four different specimens in that valuable collection of animals, which the directors of the Hudson's Bay Company have lately procured from a country unvisited but by their own servants, it appears that the change begins in October (or perhaps the latter end of September) and that it is completed in January.

We owe this knowledge of the regular gradations of colour in this animal at different intervals as the winter advances, to the very sensible attention in the company's servants, who have tied memorandums to the specimen of each animal, which inform us of the day and month in which it was caught.

If the fur of your quadruped is accurately examined, it will be found to consist of two distinct coats of hair, one of which is much more thinly scattered over the body, but is more than twice the length of the inmost covering, at the same time that it is vastly stronger.

This upper and thinner coat is composed also of hairs which are white from the top to the root, and form the winter furtout for the animal : its brown fur, therefore, never becomes white, but is concealed by the upper coat.

This additional covering seems to be absolutely necessary for the animal's preservation, as it is thereby enabled to endure the rigour of a Hudson's Bay winter, whilst at the same time the colour of the new fur being white, prevents its being distinguished by its very numerous enemies [g].

If this furtout, however, was not to fall off during the summer, it would prove the destruction of the animal.

animal: because the extraordinary heat from such a warm-cloathing would not only become highly inconvenient, but the colour also (being white) would point the animal out to its pursuers; as Mr. Graham takes notice in his very accurate catalogue, that this quadruped does not burrow.

This very sensible officer of the Hudson's Bay Company likewise adds, that this animal continues always near the same spot; that its coat is brown in summer; that they breed from 5 to 7 young ones, and sometimes twice a year: he also states, that the weight at a medium is nearly 4 lb. I am,

DEAR SIR,

Your most faithful,

humble servant,

Daines Barrington.

[g] It must be admitted, however, that the white coat during the winter is not a sufficient protection to the animal against the sagacity of the arch-enemy man.

Mr. Reinhold Forster (who is a native of Polish Prussia) informs me that Hares are found in the northern parts of Europe, when the snow is on the ground, by an exhalation of vapours from their bodies, whilst they are sitting in their form, especially if the sun happens to shine.

I can very easily conceive that such a vapour may be distinguished, as I have frequently in a frosty morning seen the air condensed, which hath issued from the mouth of so small a bird as a Robin, when in full song.

III. *An Account of the sulphureous mineral Waters of Castle-Loed and Fairburn, in the County of Ross; and of the Salt purging Water of Pitkeathly, in the County of Perth, in Scotland: By Donald Monro, M. D. Physician to the Army, and to St. George's Hospital, Fellow of the Royal College of Physicians, and of the Royal Society.*

Read Jan. 23,
1772.

AS no account of these waters has hitherto been published, I thought that the following would be agreeable to this Society.

Of the sulphureous mineral water of Castle-Loed.

Having heard many gentlemen from the county of Ross speak of these mineral waters, I wrote to Sir John Gordon, of Invergordon, and begged the favour of him to ask some physical person in his neighbourhood to send me an account of them, and likewise some of the waters in bottles; and soon after he was so obliging as to send me six bottles of the Castle-Loed, and six of the Fairburn waters, sealed and corked,

corked, and along with them a letter from Dr. Alexander Mackenzy, dated August 9, 1771, containing the following account.

“ The Castle-Loed is a strong sulphureous mineral water; when taken up from the spring, it is as pure and transparent as the clearest rock water; but if kept in an open vessel, or an ill-corked bottle, it soon becomes of a milky sort of foulness, and it loses its strong sulphureous smell in twenty-four hours.

“ The bottom of the well, and of the channel which conveys its water from thence, is black, as if dyed with ink; and the leaves of the aller bushes that fall into the well, or into its channel, soon contract a blackish colour in the water; but when taken out, and dried in the sun or shade, appear covered with a whitish dust, which is undoubtedly sulphur; for, by burning one or more on an ignited shovel, or clear live coal, they produce a blue flame, and emit a very suffocating sulphureous smell.

“ All that I can learn of the operation of this water, from some sensible people of credit and observation, who have drunk it this as well as former seasons, is, that it very sensibly increases the urine, and sometimes remarkably opens the pores; but I do not find, from the report of any, that it purges, though drunk to the quantity of three, sometimes of four, English quarts in the day. Almost every person remarks, that it whets the appetite, and sits light on the stomach. I have been told by several, that they have had head-

“ aches immediately after drinking their morning
 “ bottle, but of no long duration, nor to any great
 “ degree.

“ It is impossible to say with certainty the num-
 “ ber of cures these waters have made, or what par-
 “ ticular cases have received most benefit from
 “ using them; for every person in the county pre-
 “ scribes the water for themselves, and runs to the
 “ well, or sends for the water, for every complaint,
 “ acute and chronic.

“ I have indeed myself directed several people
 “ with various complaints to drink them. Some
 “ very foul faces have been quite cleared; and, at
 “ this time, a gentleman's son, nine years of age,
 “ with a herpes round the neck, which had proved
 “ extremely obstinate to other means, has got a per-
 “ fect cure by drinking and washing with them;
 “ and his sister, a young lady of eighteen, who, from
 “ an untoward recovery from the measles and small-
 “ pox, fell into a sort of habitual erysipelas on the
 “ face, head, breast and arms, is now using them,
 “ and, I think, with evident advantage. Some foul
 “ ulcers on the legs, and one with every appearance
 “ of a carious thigh bone, have been perfectly cured.
 “ And a servant-maid in my own family, who had
 “ been for several years, periodically in the winter,
 “ afflicted with severe rheumatic pains in her arms
 “ and shoulders, received remarkable benefit from
 “ this water, one summer; in so much, that the
 “ winter succeeding she had little or none of her
 “ rheumatic pains, and her appetite and digestion
 “ were much improved.”

So far Dr. Mackenzy. From others I have been informed, that this water has been used with success in many of those cutaneous disorders commonly called scorbutic, and in curing the itch.

In order to discover the particular contents of this water, I began to examine the bottles, which had been sent me, on Tuesday, the 10th of September, which was about five weeks after the water had been taken up from the well. The bottles were all well corked, and the tops of the bottles had been dipt into melted wax so soon as they had been corked. The water was as clear and limpid as the purest rock water. It had still a very strong sulphureous smell and taste; but it had no other but a sulphureous taste, and it made no impression, on the tongue, of sea or any other salt.

Some of it was poured into different glasses and tea-cups, and different things put into each.

Syrup of violets became slowly green.

A watery tincture of galls occasioned no particular change of colour, but brought a variegated scum, of the colour of a pigeon's breast, to the surface.

A diluted spirit of vitriol mixed smoothly, and occasioned no white cloud, nor more emotion or cloud than if it had been dropt into distilled water, only soon after a number of air bubbles collected at the bottom and sides of the glass; and the same thing happened, when some drops of the strong oil of vitriol were mixed with another parcel of the water.

Each drop of a solution of pure crystallised native fossil alkali occasioned a white cloud, and a white precipitate fell to the bottom of the glass; but each drop of a solution of salt of tartar caused a dark brown

brown-coloured cloud, with a precipitate of the same colour.

A shilling and a sixpence, put into two different tea-cups, were presently tarnished, and became of a very dark colour.

Each drop of a solution of silver in spirit of nitre, occasioned a dark brown or blackish cloud, and fell in form of a black precipitate to the bottom of the glass.

Some very white saccharum saturni turned immediately black, and precipitated in form of a black powder to the bottom of the glass.

Four pounds seven ounces and six drachms, (or lxxi ounces vi drachms) were poured into a stone basin, which was put on a sand heat to evaporate with a slow fire.

As soon as the water became warm, it lost its strong sulphureous smell, and there appeared some flakes of a dark brown light earth, which dropt to the bottom. After about one half was evaporated, a very thin pellicle was observed on the surface, which precipitated to the bottom, and when it was reduced to about a pint (lib. i.), it was filtrated through paper, and about 2½ gr. of a dark grey insipid sediment was separated. This sediment was composed of the dark coloured earthy flakes, which were observed so soon as the water had become warm, and of a small quantity of a whitish, insipid gritty matter, which had formed the very fine thin pellicle. Some of it being thrown into distilled water, and oil of vitriol dropt into it, an effervescence ensued, and the black earthy part dissolved, though the rest remained insoluble; hence the first

part, or black earth, should seem to be of the absorbent kind, the other an insoluble earth, or a tellenite. Whether the first earth was originally dissolved in the water by means of air, or whether it was only light particles of earth, which had been blown into the well, and only suspended, I shall not take upon me to determine; but, in looking at the water of another bottle, which was not used in this experiment, I observed, that although the water appeared quite transparent, yet that it contained some particles of light earth suspended; however, these might have been originally dissolved by means of air, but separated afterwards.

The remaining water was put into a small basin, and set again on a sand heat to evaporate; when it was reduced to about three ounces, a pretty firm pellicle appeared, and it was set in a cool place for twenty-four hours; at the end of which time, it was examined; and, besides the pellicle which had formed on the surface, a thin white lamellated and granulated crust had formed, and attached itself to the bottom and sides of the basin. These being all separated, the whole was thrown into a filter; and when the water had passed, and the collin was dry, there was found remaining gr. xi. of a very white, insipid gritty sediment. This sediment in the mouth feels gritty, and has no taste, being quite insipid; when some of it was put into a glass with distilled water, and a few drops of oil of vitriol mixed with it, a very slight effervescence ensued; but almost the whole remained undissolved, and appeared to be tellenite.

The

The water, being again set to evaporate, was reduced to less than an ounce, when it was again filtered, and gr. ii fs. of a residuum separated. This sediment appeared to be nearly of the same nature as the former; but, on putting it into the mouth, I thought I could perceive somewhat, though very little, of a saltish taste, and when thrown into water, it remained all undissolved.

The small quantity of water which was left, was next evaporated to dryness, and there remained in the tea-cup gr. xvii. of a yellowish matter, composed of the thin lamellæ of some salt, and a yellow unctuous or oily substance. It had rose into blisters, and emitted a very strong smell of sulphur, while it yet remained hot.

Some of the saline matter, being put into a solution of the caustic alkali in distilled water, occasioned a white cloud; and the same thing happened when some of it was mixed with solutions of silver in the nitrous acid, and of corrosive sublimate in distilled water.

In order to separate the salt from the yellow oily matter, the sediment was thrown into and dissolved in an ounce of distilled water, and then passed through filtering paper, and evaporated to a pellicle; after which, it was set in a cool place for forty-eight hours, in which time a crystallization took place, and I separated very near gr. xii. of a salt similar, in every respect, to that of Glauber; but it had still a little of the yellow oily matter adhering to it. This salt had a cool bitter taste; it dissolved easily in distilled water, and when some of the native fossil alkali was added to this solution, it remained clear; though,

though when a solution of the caustic vegetable alkali was added, each drop occasioned a white cloud. Some drops of oil of vitriol, let fall on some of this salt, occasioned no effervescence, nor raised any fumes; and when some of it was put on a red hot poker, it rose into blisters, and did not crackle. From all which I conclude, that this was a true Glauber salt.

After this salt was separated, the remaining liquor was left in the tea-cup, and, at the end of four days, it concreted into a yellow cake, which still contained a good deal of a salt; it weighed gr. ix. so that there had been a great increase of weight, from the water the salts had taken up in their crystallisation. This matter was extremely bitter, and had likewise a cooling saline taste. Some of it, put on a red-hot poker, melted, and rose into blisters; it emitted a little smoke, but did not flame; and it had such a very slight sulphureous smell, that it was doubtful whether it proceeded from the yellow matter or the ignited iron; it burnt to a black coal, which tasted somewhat saltish. Some oil of vitriol dropt on some of this matter occasioned very little effervescence, but raised a strong pungent acid smell, which I at first imagined must be that of sea salt; but, from the few marks I have observed of the existence of this salt in a perfect state in these waters, I have since thought, that it might be that of a volatile vitriolic acid, which had been formed by the union of some of the acid of the Glauber salt, with a sulphureous or oily matter, and dislodged by the addition of a fixed acid of the same kind, though perhaps there may be a pittance of a calcareous marine salt
in

in the yellow ley mixed with the Glauber salt. Having accidentally added a solution of caustic alkali to this mixture of the yellow matter, and vitriolic acid, it emitted immediately a strong smell of hepar sulphuris. This yellow matter I take to be composed of a Glauber salt, and a yellow oily matter common to almost every water, though perhaps in larger proportion in this than in many others; but whether it contains a pittance of sea salt, I think is doubtful; and what makes me still doubt the more, is my having since evaporated 44 ounces of the water, from which I obtained gr. x. of residuum, when the water was at last evaporated to dryness, which I threw likewise into distilled water, and the most of it passed the filtre, and concreted into pure crystals, resembling those of Glauber salt, leaving but a very small pittance of a yellow oily matter behind. However, it will be necessary to have a pretty large quantity of this last residuum, to determine with precision the nature of every ingredient in its composition, and the exact proportion of each.

Having tried as many experiments as I could, with the small quantity of this yellow matter I had, I last of all examined the coffin through which the salts and it had passed. After it was dry, I found that it had increased gr. ii. in weight, and that it was covered in the inside, with a yellowish powder. When part of the paper, with this powder, was lighted with a candle, and the flame extinguished, it smelt strong of sulphur; and, on rubbing a shilling with another bit of the paper, it immediately tinged it yellow, as pure sulphur would have done. From whence, I think, we have reason to conclude, that this powder

contains more or less of a true genuine sulphur, or, at least, of a sulphureous matter.

From what has been said, it appears, that this is one of the strongest sulphureous waters hitherto found in Great Britain, though I make no doubt but that there are many such which have not hitherto been examined: That, in its natural state, it is highly impregnated with a volatile sulphureous vapour, which evaporates soon when exposed to the open air, and flies off immediately when exposed to heat; and that the water then loses its strong sulphureous smell and taste, though we have the strongest reason to suspect that it still contains a sulphureous matter dissolved in it, by some means hitherto unknown to us; for it neither contains an alkaline salt nor quicklime, the two only substances we hitherto know to be capable of dissolving sulphur, and keeping it suspended in water: That it lets drop to the bottom of the well, and of its channels, a fine powder of sulphur, which adheres to the leaves and branches of trees found there.

As this water contains but very little purging salt, and does not operate by stool, sea water, or some purging salt, may be added to the first glasses drank in a morning, when purging is required. Equal parts of the ~~Castle-Lord~~ and sea water, mixed together, make a water in most respects similar to the Harrowgate; and probably will be found to answer in most cases where the Harrowgate has been found useful; and it may often be used with more advantage than the purging sulphureous waters, as they sometimes purge people of weak constitutions too freely, and weaken them too much.

Of

Of the sulphureous Mineral Water of Fairburn.

Dr. Mackenzy in his letter mentioned no more of this than that he believed it to be a weaker water, of the same nature as the Castle-Loed.

I subjected it to the same tryals, as it: on opening the bottles, it emitted a strong sulphureous smell, tinged silver, and produced nearly the same appearances as the Castle-Loed when mixed with the same substances, only it remained clear when a solution of the true fossil alkali was mixed with it; the caustic vegetable alkali occasioned a very small light, darkish cloud, and precipitated but a very small quantity of a very light sediment, owing, as appeared afterwards, to this water containing an absorbent or calcarious earth, which probably was suspended by air, and but very little if any selenite.

I evaporated lib. viii. drachm. i. scrup. i. (or 128 ounces, four scruples) of this water with a slow fire. When it was evaporated to one half, it was filtrated through paper; which operation was repeated again, when reduced to three ounces; after which the remainder was evaporated to dryness, and the solid matter left, thrown into distilled water, filtrated again through paper, and evaporated to a pellicle, and set in a cool place for the salts to crystallise.

By these operations, I obtained near gr. ii. of a dark coloured light earth, which effervesced with acids, and dissolved; gr. xv. of a white calcarious earth, which effervesced with and dissolved in the vitriolic acid;—and gr. xxiv. of Glauber salts mixed with a
 Vol. LXII. E yellow

yellow oily matter ; but I got no selenites, nor any matter which coloured silver, or I had any reason to suspect to be sulphur.

Some of the salt was dissolved in distilled water, and different liquors were dropt into different parcels of it.

Syrup of violets became immediately of a green colour.

Each drop of a solution of silver in the nitrous acid, occasioned a bluish white cloud, which fell to the bottom.

The solution of the fossil alkali mixed clear, as did a solution of salt of tartar, but each drop of a solution of common caustic alkali gave a white cloud ; some oil of vitriol dropt on a little of this salt effervesced, and emitted acid fumes, while it yet was mixed with a good deal of the yellow oily matter ; but after the salt was dissolved in distilled water, and again crystallised, and freed of most of this yellow matter, no fumes were to be observed ; and the acid smell was extremely faint when strong spirit or oil of vitriol was dropt upon it.

This, though it does not appear to be such a strong sulphureous water as the Castle-Loed, yet it may have its uses, and be serviceable to those who have not an opportunity of using the other ; and it may perhaps be useful in some cases, where the other may not agree.

Of the Salt Purging Water of Pitkeathly, in the County of Perth.

There are but few salt purging waters, which have hitherto been discovered in Scotland ; the Pitkeathly,

Pitkeathly, situated about six miles from the town of Perth, is the one in most esteem, and the most frequented.

As no particular treatise has been published on these waters, and I wished to know their particular nature and contents, I wrote to his Grace the Duke of Athol, whose seat of Dunkeld is within 14 or 15 miles of the wells, and begged the favour of him, to ask some of the physical people in the neighbourhood to examine these waters, and to send me an account of them; and in consequence thereof, his Grace was so obliging as to send me a letter from Dr. Wood, of Perth, giving the following description of them; and afterwards six bottles of the water, which arrived in spring 1771.

“ The spring rises in a very low marshy ground,
 “ undistinguishable from any other; but, by the taste
 “ of its water, it is generally believed to contain
 “ no mineral principle, but a small proportion of
 “ marine salts. It acquires somewhat of a putrid
 “ taste by keeping, but retains its purging quality;
 “ and it keeps much better in open, than in corked
 “ bottles.

“ It purges gently, and without griping. An
 “ adult person drinks commonly a bottle and a half,
 “ or two bottles, of a morning.

“ In scrophulous and scorbutic habits, it is certainly a most useful water.

“ A new spring has been lately discovered about
 “ two or three hundred yards from the old one,
 “ but its waters seem to be much of the same
 “ strength and quality as the former.”

Since receiving the above account, I wrote to Dr. Wood, and begged to know of him what proportion of sea salts these waters contained, and whether they had any mixture of a bittern in their composition; and I had the following answer, dated Oct. 17, 1770.

“ Since I received your letter, I evaporated a
 “ Scotch pint (Lib. iv.) of these waters in a white
 “ stone basin, and I obtained two drachms of a
 “ salt, which always run *per deliquium*, and would
 “ not crystallise. I shall try it again in the summer, as at this season the air, being much charged
 “ with watery particles, may have prevented the
 “ crystallisation. By dropping a solution of potash
 “ into three Scotch pints (Lib. xii.) of the waters,
 “ I got eighty five grains of a very fine magnesia.”

The six bottles of this water which were sent to me, arriving at a time when I was much engaged, they remained for several months in the hamper in which they were originally packed; and I did not try any experiment with the water till the 2d of October last. It was then clear and transparent as the purest rock water, only it seemed to have some few particles of light earth swimming through it. It had then, a fetid sulphureous smell, resembling somewhat that of a foul gun or of rotten eggs, and it tinged silver in the same way as the sulphureous waters beforementioned; and it had a sulphureous and slight saltish taste. This fetid sulphureous smell, taste, and property of tinging silver, which this as well as most other salt waters acquire by keeping, I suspect to be owing to a fermentation taking place in the water, and slightly uniting some of the fine oily matter.

matter with some of the acid of the salts which these waters contain, and thus forming a sulphureous vapour which is volatile while they remain slightly united, but which by a more intimate union would form a real fixed sulphur. From Dr Wood's account of this water, it is evident that this fetid vapour, or at least the principles which form it, are volatile; for, he says, the water keeps much better in open than in corked bottles.

Each drop of a solution of the fossil as well as of the vegetable alkali occasioned a thick white cloud, that fell to the bottom of the glass. And each drop of a solution of silver in the nitrous acid gave a milky cloud. Syrup of violets became green, and an infusion of galls occasioned no particular change of colour.

A hundred and two ounces, three drachms and a scruple (or lib. vi. unc. vi, dr. 3. scrup. i.) were put into a large stone basin, and set on a sand heat to evaporate with a slow fire.

As soon as the water was warm, it let drop a light dark coloured earth, which gathered in small heaps at the bottom of the basin, and during this time, the water threw up some air bubbles to its surface; when it was evaporated to about a pint (lib. i.) it was taken off the fire, and filtrated through paper: the coffee through which it passed, after being dried, was found to have acquired 21 grains of additional weight; though I could not collect more than 3 gr. of a stone grey coloured earth, which proved to be of the absorbent or calcareous kind, for it effervesced with and dissolved in the vitriolic acid; the remaining additional weight of the coffee, I believe, depended on some of

the salts of the water being taken up by the spongy filtrating paper.

After this, the water was again set on the sand heat and evaporated till a pellicle appeared on the surface; and during the evaporation it threw up a great number of air bubbles: after this, it was set in a cool place for three days, at the end of which time there appeared a quantity of thin lamellæ, mixed with a small granulated salt, covered with a light coloured yellowish liquor; these I separated, and threw the liquor into filtrating paper; and by these operations I got $53\frac{1}{2}$ grains of a Salt which tasted sharp and salt, besides what had been taken up by the coffin, which had increased gr. 9. in weight more than I had got of salt. This salt being put in a tea cup appeared next day white, and had contracted a little moisture, but did not run *per deliquium*.

The remaining water which was now a yellowish ley, was again evaporated to a pellicle, and I separated a quantity of white salt in lamellæ, which remained moist, till it was set in a tea cup on the sand heat and evaporated to dryness, when it weighed one drachm and 14 grains; this salt attracted more moisture than the former, and seemed at first as if it would run soon *per deliquium*; but the next day it remained in the same state.

As I imagined that both this and the salt before separated was mostly sea salt mixed with a bittern and oily matter, which prevented the crystallisation; I dissolved the whole of both in distilled water, and evaporated with a very slow fire till a crystallisation began to appear, and then set it in a cool place, and got some large perfect crystals of sea salt, and by repeating

ing this several times, I obtained a full drachm of perfect crystals, which diminished in their size as the process advanced; and afterwards a scruple more of thin lamellæ, which on examining with a magnifying glass appeared to be made up of small square crystals; there remained a small quantity of a salt ley, which probably would have yielded a few more such lamellæ.

The liquor which remained after the two first parcels of salt were separated, was next evaporated; but no pellicle appearing, the operation was continued till it was quite dry, when it formed one transparent yellow or amber-coloured salt cake, which weighed one drachm and 34 grains. This salt on being put into a tea cup, presently began to run *per deliquium*, and dissolved entirely by standing in a cupboard which was in a room where there was a fire; but the fire having been let out in the evening, and the night proving cold, I found next morning that a crystallisation had taken place, for there was a crystallised cake at the bottom of the cup, which was covered with an amber-coloured ley; it at first seemed to be all one piece, with a number of small points standing up on its surfaces; but on reclining the tea cup to a side, it then appeared to be made entirely up of a number of oblong crystals about the length of a barley-corn, but not so thick, and that the points beforementioned were the ends of these crystals. Not having time to examine them particularly in the morning, and to know their exact figure and number of sides, I set them by, till I should come home again about one o'clock; but the day proving warm, they were mostly dissolved before that time.

Oil of vitriol, dropt into a tea cup in which there was some of this ley, immediately occasioned a white firm.

firm coagulum like chalk, which was insoluble in water, and, when well washed and freed of its acid, felt gritty and was quite insipid in the mouth; this is certainly a selenites formed by the earth of this ley and the vitriolic acid.

From this account of the Pitkeathly waters, it appears that lib. vi. unc. vi. dr. 3. scr. i. besides a few grains of an absorbent or calcarious earth, contain three drachms, $41 \frac{1}{2}$ grains (besides what was lost in filtrating and other operations) of a saline matter, of which near two thirds were sea salt, the rest a bittern or salt with an earthy basis, which concreted by the force of fire into a yellow saline mass, that runs soon *per deliquium*, and crystallises though with difficulty.

The small quantity I had of residuum prevented my being able to determine with precision, the exact proportion of sea salt and of this bittern; neither was I, for the same reason, able to determine whether this bittern or ley was all made up of a calcarious marine, with an oily matter common to all waters, or whether it contained likewise a sal catharticum amarum with a vitriolic acid.

From the acid of vitriol forming an insoluble selenites with the earthy basis of this bittern, it is evident, that at least all the earthy basis is not a magnesia, such as makes the basis of the sal catharticum amarum of the shops, or what goes by the name of Epsom salts, otherwise it would have formed a salt easily soluble in water.

IV. *Extract of a Letter from Mr. George Witchell, F. R. S. and Master of the Royal Academy at Portsmouth, to Charles Morton, M. D. Sec. R. S. inclosing some Account of a Solar Eclipse observed at George's Island, by Captain Wallis; and several Astronomical Observations made at Portsmouth.*

To Cha. Morton, Sec. R. S. &c.

S I R,

Read Feb. 13, 1771. **I** Beg the favour of you to lay before the Royal Society, an extract of a letter to me from Captain Wallis, containing an observation of a solar eclipse, which happened during his stay at George's Island, from which I have calculated the difference of meridians between that place and Greenwich.

To this I have subjoined some astronomical observations, which have been made here by Mr. Bradley and myself, both before and since the building of the Observatory belonging to the Academy; which, if they should prove acceptable to the Royal Society, will induce us to take every opportunity of

continuing to transmit them, as we shall always esteem it a peculiar happiness to be able to contribute any thing, that may be deemed worthy of their notice.

I am, S I R,

Your most obedient,

humble servant,

Royal Academy, Portsmouth,
August 9, 1771.

G. Witchell.

Extract of a Letter from Captain Wallis,
June 20th, 1771.

——— “ Saturday, July 25th, 1767, being at
“ anchor in his Majesty's ship Dolphin in harbour,
“ went on shore on a low point of land, not above
“ four feet higher than the sea, and observed an
“ eclipse of the Sun, as below. Latitude, by the
“ mean of many observations, $17^{\circ} 30'$ South, lon-
“ gitude, by various observations of the distance of
“ the Sun from the Moon, between $149^{\circ} 30'$ and
“ $149^{\circ} 50'$ West from London.

“ By

	h	'	"		h	'	"
" By the master's watch at	7	6	0	} A. M. the ☉'s altitude was 8° 43' on the quadrant, } without any correction. Hence the app. time }	7	5	20
" By my watch at . . .	7	3	20				

" By the master's watch at	8	13	0	} A. M. the ☉'s altitude was 22° 52' on the quadrant, } without any correction. Hence the app. time }	8	12	12
" By my watch at . . .	8	10	12				

	h	"	"		h	'	"
" The eclipse began, by the master's watch, at . . .	6	52	30	} A. M. Apparent time, 6 51 50	6	51	50
" By my watch, at	6	49	50				

" The end of the eclipse, by the master's watch, at	8	1	48	} A. M. 8 1 0	8	1	0
" By my watch, at	7	59	0				

" Duration 1 9 10

" We were not certain of the instant of the beginning of the eclipse, from a little negligence ; but
" very certain of the end."

R E M A R K.

As the Sun's altitudes are given, without any correction, I suppose they were taken by bringing down the image of the Sun, till it appeared bisected by the visible horizon : I have therefore recomputed the time, by allowing for the dip and refraction, which, together, amount to 8'. This correction makes the apparent time of the beginning 6^h 51' 12'', and the end 8^h 0' 37'' ; hence the duration of the eclipse was 1^h 9' 25'' ; but, by a careful computation from Mayer's new Tables, the duration should have been 1^h 13' 20''½, which is almost about 4' longer than the observation affords ; but, as it is remarked that the beginning was not exactly taken, and the Moon entering very obliquely on the Sun, the defect in 4' would be but little. It seems most reasonable to attribute the whole of the error to the beginning of the eclipse. I have therefore deduced the longitude from the end, and

make it to be $9^h 55' 55''$ West from Greenwich, or $148^{\circ} 58\frac{1}{2}'$, which is $41\frac{1}{2}'$ less than the mean result of the lunar observations, which, considering all circumstances, is not, in my opinion, a very great difference for the first observations that were ever made upon this island.

Astronomical Observations made at the Royal Academy, Portsmouth.

1769. May 9th, at $8^h 13' 9''$, apparent time, Mr. Bradley observed the immersion of ζ Π^{orum} by the Moon, uncertain to a few minutes, on account of the strong twilight. The emersion was not taken.

The transit of Venus, and solar eclipse, next morning, were both observed here; but, having then no better instrument for determining the going of the clock, than an indifferent Hadley's sextant, I do not think the observations worthy of being laid before the Society; and, for the same reason, omit the observations of the comet.

1770. April 7th, at $11^h 23' 33''$, apparent time, by Mr. Bradley's observation, the Moon occulted ϵ Ω^a . My time was within $2''$ or $3''$ the same; but we did not observe the emersion. This occultation was observed both at Greenwich, by Mr. Maskelyne, and at Oxford, by Professor Hornsby; by comparing which, it appears that this place is West of Greenwich $4' 24\frac{1}{2}''$ of time, and that Oxford is West of Greenwich $4' 58\frac{1}{2}''$.

1770. April 28, at $9^h 48' 13''$, apparent time, Mr. Bradley and I, both at the same instant, observed the immersion of $\zeta 8^i$ by the Moon. The emersion was not taken. By comparing this with Mr. Maskelyne's observation, our longitude comes out $4' 23''$ West from Greenwich.
1770. July 21st is marked for an eclipse of Jupiter's fourth satellite in the Nautical Almanac; but the *Connoissance des Temps* notes it as a conjunction only, and remarks, that the satellite would raise the shadow, without disappearing; which we found to be true, for we both saw the satellite the whole time which is marked for its duration, though, at the middle, it appeared extremely faint.

These observations were made before our Observatory was finished; but that being completed in the month of September, and furnished with an excellent (though small) mural quadrant and transit instrument, both made by that eminent artist Mr. John Bird, we began to observe meridian transits, from which I shall select those that were made for determining the solstices, and the oppositions of the three superiour planets, which I shall transcribe, just as they were taken, excepting only making the necessary allowance for the error of the line of collimation.

Observations for determining the Solstices.

		App. zen. dist. of the ☉'s V.L.			App. zen. dist. of the ☉'s L.L.			Baro- meter.	Thermo- meter.	Hence the app. ren. dist. of the Sun's center.		
		°	'	"	°	'	"			°	'	"
1770.	Dec. 9.	73	21	40.3	73	54	5.1	29.76	37	73	37	52.7
	12.	73	36	41.8	74	9	25.0	29.78	46	73	53	3.4
	14.	73	44	22.0	74	16	58.2	29.87	44+	74	0	40.1
	21.	73	56	41.0	74	29	13.1	29.37	43+	74	12	57.0
	22.	73	56	20.0	74	29	8.1	29.94	38½	74	12	44.0
	29.	73	42	27.2	74	15	0.4	29.62	48½	73	58	43.8
1771.	Jan. 3.	73	18	18.0	73	51	3.6	29.23	46	73	34	40.8
<hr/>												
1771.	June 18.	27	5	45.4	27	37	42.2	29.82	64+	27	21	43.8
	19.	27	4	52.7	27	36	32.1	30.03	66½	27	20	42.4
	20	27	3	52.0	27	35	44.4	30.11	71+	27	19	48.2
	21.	27	3	33.6	27	35	26.0	30.12	70	27	19	29.8
	22.	27	3	51.0	27	35	29.4	30.11	67½	27	19	40.2
	24.	27	5	17.1	27	37	3.5	30.06	67	27	21	10.3
	25.	27	6	43.2	27	38	32.0	30.04	72½	27	22	37.1

By comparing these observations together, I make the
 true zenith distance of the Sun's center, at the
 Winter Solstice, to be } 74 16 13.4
 And at the Summer Solstice 27 19 51.6

Therefore, the distance of the tropics 46 56 21.8
 Half } 23 28 10.9

By Mr. Mayer's Tables, the decrement of the obli-
 quity, in three months, is } 0.1

Hence the mean obliquity, December 21, 1770 . . . 23 28 11.0
 And June 21, 1771 23 28 10.8

Therefore, the latitude of the Observatory, by these
 observations, is } 50 48 2.4 North.

The above observations were chiefly taken by Mr. Bradley. Those which
 follow are what I took about the time of the late oppositions of the superior
 planets, in which, as well as the preceding observations, the apparent zenith
 distances are those which were taken by the 96th arc, on which we chiefly
 depend, though the difference between the two arcs seldom arises to more
 than three or four seconds.

For

For the Opposition of the ☿ and ♂.

	Time per clock.	Meridian Transits.	App. zen. dist.	Barom.	Therm.
Days.	h ' "		o ' "		
1770. Dec. 10.	5 18 58½ 20 51— 36 49— 43 19	β Leporis passed the middle wire. ♂ Orionis. Mars α Orionis.	24 41 6.0	29.87	28½
13.	5 12 24— 31 35 43 21+ 6 1 37	β Tauri. Mars α Orionis. γ Geminorum.	24 38 11.4	29.76	42½
16.	5 26 22½ 43 21	Mars α Orionis.	24 36 55.5	29.80	47½
17.	5 12 24½ 24 40 43 21	β Tauri. Mars α Orionis.	24 36 15.6	29.91	43
20.	5 4 7— 19 40: 37 29+	Rigel. Mars α Orionis.	14 36 25.6	29.16	43.

For the Opposition of the ♄ and ♃.

	Time per clock.	Meridian Transits.	App. zen. dist.	Barom.	Therm.
Days.	h ' "		o ' "		
1771. Febr. 1.	9 1 51+ 16 26—	Saturn passed the middle wire. α Hydræ.	32 48 20.8	29.89	42
2.	9 1 32 16 26½	Saturn α Hydræ.	32 46 53.4	30.07	42+
7.	8 32 58+ 59 57½ 9 29 4+	γ Leonis. Saturn α Leonis.	32 39 27.6	30.04	28
10.	8 59 1½ 9 16 32 29 5+	Saturn α Hydræ. γ Leonis.	32 35 9.5	29.72	18½

For

[4°]

For the Opposition of ☿ and ♃.

	Time per clock.	Meridian Transits.	App. ren. dis.	Latom.	Therm.
Days.	h ' "		h ' "		
1771. July 12.	16 15 41	Antares passed the middle wire.			
	18 0 21½	♄ Sagittarii.			
	9 44	♄ Serpentis.			
	14 7½	♄ Sagittarii.			
	19 39 8½	Jupiter	72 40 43.8	29.87	57½
13.	7 28 52½	☉'s preceding limb.			
	31 8+	☉'s following limb.			
	19 38 34	Jupiter	72 41 52.5	30.18	57+
14.	7 30 53	☉'s preceding limb.			
	35 10+	☉'s following limb.			
	19 37 59	Jupiter	72 43 22	30.26	61
15.	7 36 55	☉'s preceding limb.			
	39 12	☉'s following limb.			
	16 15 35+	Antares.			
	19 37 23	Jupiter	72 44 57.1	30.12	59
17.	7 44 56	☉'s preceding limb.			
	47 12+	☉'s following limb.			
	16 15 31	Antares.			
	19 36 14½	Jupiter	72 47 50.7	29.73	65
23.	8 8 46	☉'s preceding limb.			
	11 0½	☉'s following limb.			
	16 15 19½	Antares.			
	19 32 48	Jupiter	72 55 47.3	32.21	60½

An Occultation of ♎ Libræ by the Moon.

	App. time.	
Day.	h ' "	
1771. April 2, at	13 7 9½	♎ Libræ emerged from the Moon's limb. Mr. Bradley makes the time 3" later. The immersion was not taken.

The

The Moon's Passage over the meridian, near the φ .

	Time per clock.	Meridian Transits.	App. zen. dist. of the φ .	Barom.	Therm.
Day.	h ' "		h ' "		
71. April 28.	2 19 54	Sun's preceding limb passed the second wire		29.74	44
	24 4	Sun's following limb passed the fourth wire			
	14 17 23.1	Moon's preceding limb passed the middle wire	Upper limb 66 13 13.0		
	19 40.1	Moon's following limb	Lower limb 66 45 43.1		

It being very clear this evening, when the Moon passed the meridian, we were in hopes of getting a good observation of the lunar eclipse, which happened soon after; but the air did not long continue in that state, but became so hazy, that we could not get any certain observations.

V. *Extract of Mr. T. Barker's Meteorological Register at Lyndon in Rutland, in a Letter to James West, Esq; Prof. R. S.*

S I R,

Read Feb. 13, 1772. **I** Have, according to your desire, sent you, on the other side, the quantity of rain which fell last year, and have added an abstract of my observations of the barometer and thermometer, and a general account of the weather here. And, with all proper regard,

I am, S I R,

Your humble servant,

Lyndon,
Jan. 18, 1772.

T. Barker..

Barometer.				Thermometer.						Rain.
				Within door.			Abroad.			
Year.		Baromet.	Mean.	High.	Low.	Mean.	H.	Low.	Mean.	
Jan.	Morn.	28.70	29.39	41	29	36	44	16	27	1.418
	Aftern.			48	30	36	50	23	31	
Feb.	Morn.	28.90	29.58	43	27	38	43	1	29	0.932
	Aftern.			49	30	39	57	25	39	
Mar.	Morn.	29.06	29.67	46	33	39	57	19	31	0.909
	Aftern.			47	34	40	57	37	39	
April	Morn.	29.03	29.79	50	37	43	57	25	34	0.976
	Aftern.			51	37	45	63	33	40	
May	Morn.	29.04	29.49	62	45	54	61	41	50	0.658
	Aftern.			64	46	56	76	57	62	
June	Morn.	29.04	29.10	62	57	57	59	46	52	1.588
	Aftern.			63	57	59	72	50	61	
July	Morn.	29.11	29.61	61	56	61	62	51	55	1.013
	Aftern.			71	57	63	77	57	67	
Aug.	Morn.	29.75	29.67	61	56	59	62	49	54	2.131
	Aftern.			70	57	61	76	56	65	
Sept.	Morn.	30.00	29.28	66	53	57	68	39	49	1.155
	Aftern.			64	54	58	66	53	56	
Oct.	Morn.	29.16	28.53	58	47	51	57	33	43	4.072
	Aftern.			59	48	52	62	44	52	
Nov.	Morn.	29.28	29.72	52	40	45	51	26	38	0.792
	Aftern.			53	41	46	56	31	44	
Dec.	Morn.	29.93	28.34	50	40	44	47	25	38	1.927
	Aftern.			49	40	44	50	35	44	
										17.586

That very wet season, the last quarter of 1770, ended about Christmas, and except three or four warm days, with some thunder, the beginning of January 1771, it was frosty above another quarter of a year. No frost, indeed, continued steady much above a fortnight together, but they were remarkably sharp, (particularly February 12, the Thermometer abroad was lower than I have seen it in above twenty years.)

years.) The intervals between the frosts were short, and often frosty mornings, and a settled frost as late as the end of March ; so that there were but few mornings, till April 20, but were more or less frosty. The effects were, that garden-things, turnips, &c. were very much destroyed ; bays, arbutus, myrtles, fig-trees, and other tender things, were killed down to the ground, and even most of the common furz ; and there were scarce any signs of spring to that time, and the winter corn was very thin.

Then the weather grew milder, and in May warm, and there came a pleasant, but cool and dry, summer, and often windy ; so that the grass was short, and the crops of hay small : but whether it was from the ground having been so soaked in winter, the coolness of the summer, or two fine rains in the middle of June and August, the ground was never so much burnt as it sometimes is. Every thing was, and continued, very backward ; the hottest part of this summer was the middle of July ; after which, though there were several fine showers, the ground continued to burn till toward the middle of August, when some rains made the grass to grow again ; yet it began rather to burn again, in some places, in September. The harvest was very late this year, especially the wheat, which both eared and ripened after the barley, and most of it was reaped in September. Both hay and harvest were well got in, and the crops were well eared, but much of the wheat and rye continued very thin ; which was too much to be feared, after so bad a seed time, and severe a winter.

October was a wet and windy month, but that did not hurt ; after so dry a summer. The wheat seed time

time was very fine; and the end of October, November, and part of December, were chiefly fair, fine and mild, saving much of people's fodder, which is scarce, and the ground is much drier than usual at the time of year. (On the other hand, in some parts of Northumberland, Cumberland, and Durham, there were at this time some such terrible rains, as made prodigious and destructive floods.) It continued open and mild to the end of the year; but part of December was wetter, making the ground dirty, yet not deep.

W. I. *Directions for using the common Astrometer, taken from a Paper in the late Dr. Bradley's Hand-writing; communicated by Nevil Maskelyne, Astronomer Royal, and F. R. S.*

Read Feb. 20, 1772. MICROMETERS, as first contrived, being only adapted to the measuring small angles, as the diameters of the Sun and Moon, or other planets, and taking the distance of such objects as appeared within the aperture of the telescope at the same time, were not of so general use as those which are contrived not only to answer the ends that the first inventors aimed at, but likewise, to take the difference of right ascension and declination of such objects as are farther asunder than the telescope will take in at once, but which pass through the aperture of it at different times. Mr. Cassini first made use of threads intersecting one another at half right angles for determining the difference of right ascensions and declinations of objects near the same parallel; and this apparatus being simple and easily procured is of very great use to such as are not provided with a micrometer made according to the late improvements. But, where such a one is at hand, that method however curious need not be made use of, the micrometer

meter serving for the same purpose with greater exactness. It was for this reason indeed that the late alteration in the form of the micrometer was made, they being before not so convenient for making such sort of observations, both hairs being usually moveable, and no provision being made for setting the hairs parallel to the diurnal motion of the objects to be observed; both which inconveniencies are avoided in the present micrometers.

The micrometer, as now contrived, is not only of use in measuring small angles or distances between such objects as appear within the aperture of the telescope at the same time, but likewise in taking the difference of right ascension and declination between stars and planets, &c. which in their apparent diurnal motion follow one another through the telescope if kept in the same situation. In making the first kind of observations, turn the short tube which carries the eye glass and micrometer, &c. till the cross thread (or that which cuts the parallel threads at right angles) lies parallel to a line passing through the objects whose distance is to be measured, and then by raising or depressing the telescope by help of the stand bring the objects to appear upon or near the cross thread, and one of them just to touch the first parallel thread: then turn the index of the micrometer till the moveable thread touches the other object, and the number of revolutions and parts of a revolution shewn by the index, turned into minutes and seconds by the table made as hereafter directed, will be the apparent angular distance of those objects. It is here supposed, that the threads exactly close, so as to touch each other when

when the index stands at the beginning of the divisions: for, if they do not, there must be an allowance made in every observation; to avoid which, it is always best to adjust the threads to the beginning of the divisions when they are first put on; for which purpose the holes in the little plate which carries the moveable thread are made oblong to give room to move it as occasion requires, before it is pinched hard by the small screws which fasten it to the moveable arm, through which the long screw passes. The other parallel thread, which I call the fixed one, must be first adjusted by setting its edge exactly over the two marks made on each side the short diameter of the aperture in the broad plates, and the cross thread must be likewise set to agree with the strokes made on each side the longest diameter, and then the intersection of the cross thread and the first parallel one will be the center of the motion given to the outer plate of the micrometer (to which the great screw index and threads are fastened) by the worm, by turning of which the first parallel thread may easily be made to lie parallel to the apparent motion of any object in order to take the difference of declination and right ascension from any other that follows through the aperture of the telescope.

This contrivance is of very great use to make a star, &c. move true along the first parallel thread, which is absolutely necessary in order to take the true difference of right ascension and declination between it and any other that follows. Without this contrivance it is very difficult to make a star move exactly upon the thread, and it can only be done by repeated

peated trials, which may sometimes take up a great deal of time.

If therefore a star is made to move on the parallel thread just at the cross, and (the telescope continuing fixt in the same position) it is afterwards near its going out of the aperture found not to be upon the thread, that must then be brought to the star by the help of the worm, and then the thread will lie parallel to the diurnal motion of the star in that part of the heavens, and consequently the cross thread will represent a meridian, and the others parallels of declinations, and the difference of time between the passage of the star at the cross wire (which was made to move along the thread), and the transit of any other star, &c. over the cross thread which represents a meridian, turned into degrees and minutes, will give the difference of right ascension. And, if the moveable parallel thread be brought, by turning the index, to touch the other star about the time of its passage over the cross thread, then the number of revolutions and parts shewn by the index (turned into minutes and seconds of a degree by the table) will be the difference of declination between the two stars. If the star is made to pass along the fixed thread so as to seem perfectly bisected, there must be an allowance made for the semidiameter of the thread or wire, because I suppose the index to be adjusted as before to the inner edges of the wires; but it may, if it is found convenient, be adjusted to the middle of the threads, or else correction may be made in the observed distance.

In taking any angle, it is convenient that each of the parallel threads be about the same distance from

the middle of the aperture of the eye-glass; and for this reason the whole micrometer is contrived to slide to and fro, as the case requires. The same motion is also of use in taking the difference of right ascension and declination, by sliding the fixt parallel thread (on which the preceding star is brought to move) towards one side of the eye-glass; for by that means a greater angle may be taken in between the parallel threads, if need be. And it must always be remembered that the moveable parallel thread should be set either north or south of the other, according as the following star is expected to be really south or north of the preceding.

In making an observation, either the inner or the outer edges or the middle of the wires may be brought to touch the objects; but then, it must be remembered to allow something for the thickness of the wire, in case the observation be not made from that part to which the index is adjusted. In observing the diameters of the sun moon or planets, it may perhaps be most convenient to make use of the outer edges of the threads, because they will appear most distinct when quite within the limb of the planet; &c. but if there should be any sensible inflection of the rays of light in passing by the wires, this would be best avoided by using the inner edge of one wire and the outer edge of the other. And in taking the distance or difference of declination between two stars, &c. the middle of the threads may perhaps be most convenient: but, however the observation is made, due correction must be allowed for the thickness of the wire, if requisite.

The

The difference of declination of two stars, &c. may be observed with great exactness, because the motion of the stars is parallel to the threads; but in taking any other distance, the motion of the stars being oblique to them is a great impediment, because if one star be brought to one thread before the eye can be directed so as to judge how the other thread agrees to the other star, the former must be somewhat removed from its thread, so that in this sort of observations the best way of judging when the threads are at the proper distance is by frequently moving the eye backwards and forwards from one to the other: this method must chiefly be made use of when the distance of the objects is pretty large, and the motion or rowling of the eye great.

The micrometer is so contrived that it may be applied to telescopes of different lengths; but then, there must be a table for each telescope, by which the revolutions of the screw may be turned into minutes and seconds of a degree. In order to this it is necessary that the threads of the micrometer should be placed exactly in the common focus of the object-glass and eye-glass, that is, where the images of objects seen through the telescope are distinctly formed. The readiest way of doing this is, first to slide the micrometer into the grooves fixt to the short brass tube, which carries the whole apparatus of eye-glass, &c. and then to draw the eye-glass out by means of its sliding work, till the threads of the micrometer are in its focus, which is known by their appearing most distinct, &c. Then thrust the short tube before-mentioned into its proper place, as for

as the shoulders of the brass work will admit, and place the object glass in its cell, and looking through the telescope at some very distant object slide the wooden tube in or out till you make the object appear most distinct, or till it has the least motion upon the threads when the eye is moved to and fro; for then the threads of the micrometer will be in the common focus of both glasses, and that will be the proper distance that the object-glass ought always to be at from the threads; and there should be made some mark or ketch in the wooden tube in order to set it always at the same distance.

The proper distance of the threads from the object-glass being thus settled, the table for turning the revolutions, &c. of the screw into angles or minutes and seconds of a degree may be made several ways; but as good and easy a method as any is carefully to measure how many inches and parts of an inch the object-glass is distant from the threads, and with the same scale to find also how many inches and parts of an inch an hundred, &c. revolutions or threads of the screw of the micrometer are equal to; then, making the first distance radius, the last will be the sine or tangent of an angle answering to 100 revolutions. And having the angle answering to 100 revolutions, the angle for any other number will be easily known and set down in the table, as also the parts of a revolution; for in small angles, such as can be observed with the micrometer, their sines tangents or cords are nearly in the same proportion with the angles themselves. The distance before-mentioned (to be used as radius) ought strictly to be taken from the threads to a point

point within the object-glass about one third of its thickness from that surface which is towards the wires, if the glass be, as usual, equally convex on both sides; but if the focus of the object-glass is pretty long and its thickness not great, the error that can arise by measuring from any part of the object-glass will become insensible as to the alteration in the angle.

The table for the micrometer may likewise be made by setting up two marks at a distance on the ground, and observing with the micrometer the revolutions, &c. which they subtend when seen through the telescope, and then computing the angles those objects subtend at the object-glass, by measuring their distance from each other and from the object-glass. The like may also be done by opening the threads to any number of revolutions, and then making a star move exactly upon the perpendicular thread, and noting the time it is passing from one parallel thread to the other; for that time turned into minutes and seconds of a degree, by allowing for the star's declination and going of the clock, &c. will be the angle answering to the number of revolutions; from which the whole table may be made. This method perhaps might be most advantageously practised in stars near the pole, where the apparent motion being slow a second in time will answer to a much smaller angle than towards the equator. But I believe, upon trial, the first method will be found most easy and practicable, especially if the scale made use of be well divided.

VII. *A Letter from Mr. John Reinhold Forster, F. R. S. to William Watson, M. D. giving some Account of the Roots used by the Indians, in the Neighbourhood of Hudson's-Bay, to dye Porcupine Quills.*

N^o 2, Somerset Stable-yard, Strand,
Jan. 16, 1772.

S I R,

Read Feb. 27,
1772.

AMONG the curiosities presented by the Hudson's Bay Company to the Royal Society, is a small parcel of porcupine quills, dyed by the wild natives, some red and some yellow, together with the roots of some plants they use for that purpose.

I examined them carefully, at your desire, and found that they are probably of the same kind with those mentioned by Prof. Kalm, vol. iii. p. 14. and 160 of the English translation. The one root, dying yellow, is called by the French in Canada, *Tifavoyanne jaune*; the other, dying red, has the name of *Tifavoyanne rouge*. Prof. Kalm declares the latter to be a new plant, belonging to the genus of *Galium*, and received by Dr. Linnæus in his *Species Plantarum*, p. 153. by the specific name of *Tinctorium*,

on account of its dying quality. It grows in woody, moist places, in a fine soil. Kalm observes, " that
 " the roots of this plant are employed by the In-
 " dians in dying the quills of the American Por-
 " cupine red, which they put into several places of
 " their work: air, sun, and water, seldom change
 " this colour. The French women in Canada
 " sometimes dye their cloth red with these roots,
 " which are but small, like those of the *Galium*
 " *luteum* or yellow bedstraw."

Dr. Linnaeus describes this plant, as having six narrow linear leaves at each knot of the stem, and four at the branches; commonly two flowers are on each stalk, and its seeds are smooth. The roots, when dry, are of the thickness of a crow quill, brown on the outside, and of a bright purple red, when broken, on the inside.

The second plant, or the *Tifavoyanne jaune*, is according to Prof. Kalm, vol. iii. p. 160. " the
 " threeleaved Hellebore (*Helleborus trifolius* Linn.)
 " grows plentifully in woods, in mossy, not too wet,
 " places. Its leaves and stalks are employed by the
 " Indians to dye yellow several kinds of their work,
 " made of prepared skins. The French learned
 " from them to dye wool and other things yellow
 " with this plant."

Among the roots sent as a specimen from Hudson's-bay, I found several leaves, which I separated, and found the plant undoubtedly to be the threeleaved Hellebore.

In the 4th vol. of Dr. Linnaeus's *Amœnitates Academicæ* is a figure of this plant, which upon comparison I found by no means to be accurate: for
 the

the leaves in our specimens, and in those collected by a gentleman who favored me with the sight of the plant, are far more pointed, than in the engraved figure. The stalks have constantly but one flower.

The dyed porcupine-quills sent along with the roots from Hudson's-bay, are of the brightest red and yellow: and this circumstance suggested to me the thoughts of trying whether these roots might not be usefully employed in dying. I mentioned it to you, and was encouraged to make such a trial, as the small quantity of the roots would permit.

I boiled a piece of flannel in a solution of half salt of tartar and half alum: the wet flannel was hereupon put into the decoction of the threeleaved Hellebore-roots, and boiled in it for the space of about 12 or 15 minutes; the flannel, when extracted, was dyed with a bright and lasting yellow dye. A white porcupine quill, boiled in the same decoction, became nearly of as bright a yellow, as those sent over from Hudson's-bay. This experiment made me believe, that I had hit upon the right method of dying with the threeleaved Hellebore; and will, I hope, prompt the directors of the Hudson's-bay Company to order larger quantities of this root from their settlements, as it will no doubt become an useful article of commerce.

The flannel, boiled in salt of tartar and alum as above-mentioned, was likewise immersed and boiled for nearly the same space of time as in the former experiment, in a decoction of the root of the *Galium Tinctorium*, but it would dye only a dull and faint red. A porcupine quill boiled with it became yellow, but by no means red. This operation

tion convinced me, that the Indians must certainly have some method or other to extract the bright and lasting colour, which I could not perform. They use perhaps the root quite fresh, which circumstance probably makes them succeed in their dying process. If it could be brought about, to extract and afterwards to fix on wool the dye of this root, it would, no doubt, on account of its bright colour, be a valuable acquisition for our manufactures : and I do not in the least doubt of the probability to succeed in the attempt, as the wollen stuffs are animal substances as well as the porcupine quills, and therefore easily susceptible of any dye.

The directors of the Hudson's-bay Company will, we hope, order their servants at the settlements to examine carefully and minutely, the method employed by the Indians in dying red with this root; and to send an account thereof, and greater quantities of this root over, that several chemists may be enabled to make experiments at large with them; for often, in dying, the experiments will not succeed, when tried in small quantities.

The wild inhabitants of North America are certainly possessed of many important arts; which, when thoroughly known, would enable the Europeans to make a better, and more extensive use of many unnoticed plants, and productions of this vast continent, both in physic, and in improving our manufactures, and erecting new branches of commerce.

To give an instance of this, I will only mention, that the Spaniards of Mexico have but lately learnt of the inhabitants of California, the art of dying

the deepest and most lasting black, that ever was yet known. They call the plant they employ for that purpose Cascalote; it is arboreous, with small leaves and yellow flowers; its growth is still slower than that of an oak; it is the least corrosive of all the known substances employed in dying, and strikes the deepest black; so that, for instance, it penetrates a hat to such a degree, that the very rags of it are thoroughly black. The leaves of the Cascalote are similar to those of the Husiaoke, another plant likewise used for dying black with, but of an inferior quality. The latitude of California lets us hope, that the country near the Mississippi, or one of the Florida's, contains this Cascalote, the acquisition of which would be of infinite use in our manufactures.

Were Natural History thus employed in applying the natural productions for procuring the necessities, or adding to the comforts and ornaments, of human life, it would for the future free this science from the vulgar opinion, that it is merely speculative, and incapable of being of the least utility in common life; a prejudice which gains more ground by the injudicious and unprofitable manner, now chiefly in vogue, in studying this branch of human knowledge; and which might be removed, if powerful trading companies would encourage the efforts of the naturalist, by enabling them to search the treasures of nature, in the various countries subject to the British Crown, and connected with its subjects by trade and commerce. Pardon, Sir, that I detain you so long on a point of which you are so well convinced, and
which

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which you have frequent opportunities to convince others of. I am, with the truest regard,

S I R,

Your most obedient,

humble servant,

John Reinhold Forster.

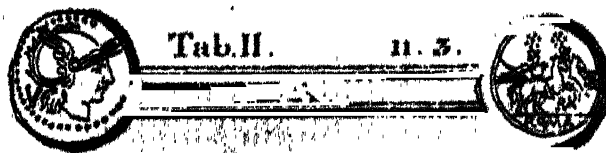
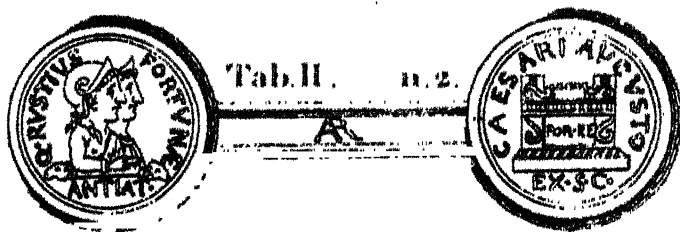
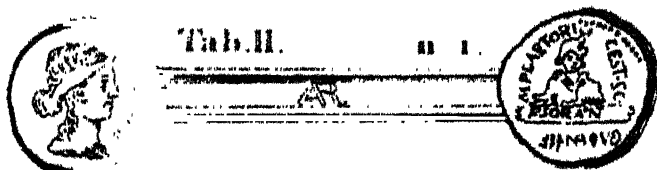
VIII. *An Account of a Subærated Denarius of the Platorian Family, adorned with an Etruscan Inscription on the Reverse, never before published or explained. In a Letter to Mathew Maty, M. D. Sec. R. S. from the Rev. John Swinton, B. D. F. R. S. Custos Archivorum of the University of Oxford, Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Dear Sir,

I.

Read Feb. 27, 1772. **T**HE piece I have undertaken to consider here is a subærated (see TAB. II. n. 1.) denarius of the Platorian family, which lately fell into my hands. It exhibits on one side a female head, representing the goddess Libera, or Proserpina, according to M. (1) Havercamp, before which stand the letters **PCOSINI**, P COSINI, very ill preserved. On the reverse, we discover a bust of the goddess SORS, on a sort of basis, adorned with the inscription F SOR ANT, or rather AN; under which, in the exergue, appear the Etruscan letters **✓✓✓NFVQ, FIR,**

(1) Sig. Havercamp. *Commentar. in Fam. Roman. Numism. Omn. &c.* p. 313, 314.



or rather FVR, ANTIE, *i. e.* FORS, FORTVNA, or SORS, ANTII, or ANTIAT', equivalent to the Latin inscription above it. The Etruscan elements seem rather better preserved than the Latin. The coin is, however, in but indifferent conservation, though pretty much of the thin silver plate remains still upon it. I must not omit observing, that it has never yet been communicated to the learned world.

II.

The symbol on the reverse here is the same that occurs on the reverses of two or three (2) other consular coins of the Platorian family, with the word SORS attending it. The Latin inscription, on the piece before me, is extremely similar to one upon a denarius of the Rustian family, now in my small collection, a draught of which may be seen in the plate (3) here referred to. The symbol there is a double Fortune, or rather two galeated Fortunes, which were considered as deities by the Romans. The divinity SORS, on the coins of the Platorian family, similar to mine, is asserted by (4) Vaillant and Havercamp to be the SORS, or rather one of the SORTES, worshiped in the temple of the SORTES at Præneste; whereas it was the SORS, or rather one of the SORTES, adored in the temple of those deities at Antium, as we find demonstratively proved by the coin I am attempting to explain. The whole

(2) Sig. Haverc. in *Fam. Plator.* Tab. I. n. 1, 2, 3.

(3) See TAB. II. n. 2.

(4) I. Vaill. *Num. Antiqu. Familiar. Romanar.* p. 238, 239, 240. Amstelædami, 1703. Sig. Haverc. *Comment. &c.* p. 324.

superstructure, therefore, erected by those two learned men on that supposition must necessarily fall to the ground.


III.

The Etruscan inscription, on the reverse of my denarius, in the exergue, seems to allude to a passage in Tully, relative to the origin of those deities denominated SORTES by the Romans, and to be illustrated by, as well as to throw some light upon, that famous passage. As this point is extremely curious, I shall beg leave to transcribe the whole passage, which has been handed down to us (5) in the following terms:

“ Numerium Suffucium Prænestinorum monumenta
 “ declarant, honestum hominem ac nobilem, somniis
 “ crebris, ad extremum etiam minacibus, cum jubere-
 “ tur certo in loco, filicem cædere, perterritum visis,
 “ irradientibus suis civibus, id agere coepisse: itaque
 “ perfracto saxo Sortes erupisse, in robore INSECVPTIS
 “ PRISCARVM LITERARVM NOTIS. Is est hodie
 “ locus septus religiose propter Jovis pueri, qui lactens
 “ cum Junone, Fortunæ in gremio sedens, mammam
 “ adpetens, &c.” “ In some of the antient monu-
 “ ments of Præneste, mention is made of one Nu-
 “ merius Suffucius. This man, who was one of the
 “ most considerable and most venerable persons in
 “ his city, both for his probity and noble extraction,
 “ was admonished, in different dreams, and at last
 “ with terrible menaces, to go to a certain place in
 “ Præneste, and there cut a flint. Being terrified.

(5) Cic. *De Divinat.* Lib. II. c. 41.

“ with







“ with these frequent visions, he obeyed. He came
 “ to the place appointed, and there, in the presence
 “ of several of his fellow-citizens, who laughed at his
 “ attempt, tried to cut a flint; which, to the great
 “ surprize of the spectators, gave way to the edge of
 “ the knife. And out of the body of the flint drop-
 “ ped several pieces of wood, (or rather oak) each of
 “ which had an inscription IN THE ANTIEN-
 “ T CHARACTERS. The place where this prodigy was
 “ performed is now walled in, because in it is very
 “ religiously kept an image of Fortune, holding Ju-
 “ piter and Juno, represented as infants, in her arms,
 “ &c.” As I have formerly (6) proved that THE ANTI-
 ENT CHARACTERS OF ITALY, or the PRISCARUM
 LITERARUM NOTÆ of the Romans, were the Etrus-
 can letters; and as the inscription formed of those
 characters, mentioned by Tully, in the passage here pro-
 duced, cannot well be supposed (7) to have contained
 any other word than , FIR, or rather
 FVR, applicable to the deity, or deities, so called,
 and worshiped, both at Antium and Præneste; we
 may fairly suppose the Etruscan inscription before me
 to have glanced at the celebrated passage just pro-

(6) *De Præcis Romanorum Literis Dissertat.* Oxoniæ 1746.
Philosoph. Transact. Vol. LXI. p. 88, 89.

(7) This must be allowed extremely probable, as the pre-
 tended origin of the LOTS, mentioned by Tully, must have
 been supposed prior to the foundation of their temples at Antium
 and Præneste, and therefore the original inscription was only, in
 all probability, supposed to have been either FIR, or FVR;
 though, after the erection of those temples, the deity or deities,
 now in view, might have been denominated F SOR ANT,
 and FOR ANT, on antient Roman coins. Vid. J. Vaill. & Sig.
 Haverc. in *Fam. Plator. et Rust.*


duced, and consequently that this passage and my explication of that inscription may be presumed mutually to support and illustrate each other.

IV.

The first of the Etruscan elements, on the reverse of my coin, , is apparently that letter in the Etruscan alphabet which, in power, is equivalent (8) to F, or PH, though the character here differs somewhat from all the forms of that element that have hitherto occurred to me on the Etruscan monuments. The second is either I, or, as I am more inclined to believe, V. That it ought rather to be considered as V, seems to me to appear from the obliquity of its position, in respect of the first letter; which seems to indicate the side of the V next to that letter to have been effaced, by the injuries of time. The third is undoubtedly the antient Tuscan , (9) or R, somewhat blotted, or blurred.. The fourth and fifth manifestly form the monogram , or AN, which has not yet occurred to me on any other Etruscan* monument. The sixth, seventh, and eighth,   , are evidently equivalent

(8) Anton. Francisc. Gor. *Mus. Etrusc.* Vol. II. p. 416, 417. Florentiz, 1737. (9) Id. *ibid.* p. 412, 417.

* From this instance, as well as others, that might easily be produced, it appears, that the Etruscans sometimes made use of monograms, as well as the Greeks, Romans, and Phœnicians. As the Romans, therefore, seem to have used monograms before the commencement of any intercourse with the Greeks, as is rendered probable by the very antient inedited quinarius here referred to (see *TAB.* II. n. 3.), which was, as I conceive, struck before the close of the fifth century of Rome; I am inclined to believe, that they borrowed this manner of writing from the Etruscans.

(10) to the Latin, or Roman, letters TIE. The whole inscription is therefore , FIR, or rather FVR, ANTIE, SORS, FORS, or FORTUNA, ANTII, altogether equipollent to the Latin inscription above it. That FVR ANTIE, in the antient Etruscan language, ought to be rendered, in Latin, FORS, SORS, or FORTVNA, ANTII, is apparent, from one of the Tables of Gubbio ; which, according to (11) Sig. Olivieri, exhibits the words AGRE TLATIE, equivalent to AGRI LATII, and is directly in point. Hence we may conclude, that FIR, or rather FVR, answers to the FORS, or FORTVNA, of the Latins, the Etruscans using constantly V for O ; and consequently that פור, PVR, PHVR, or FVR, in (12) Hebrew, or, as some will have it, in the (13) antient Persic, FIR, or FVR, in Etruscan, and FORS, or FORTVNA, in Latin, denoted the very same thing.

V.

That SORS, or SORTES, and FORTVNA, probably the same deity, (14) were worshiped both

(10) Anton. Francisc. Gor. ubi sup. p. 407, 409, 414, 417

(11) *Una Lettera del Signor Annibale degli Abati Olivieri, &c. Al Signor Abate Barthelemy, &c.* p. 42. In Pesaro, 1757.

(12) Esth. III. 7.

(13) Val. Schind. *Lex Pentaglot.* p. 1432. Hanovizæ, 1612.

(14) Hence we find the celebrated temple of the LOTS, or SORTES, at Prænestæ to have been also denominated the temple of FORTUNE ; nor do I doubt but the famous temple of the LOTS or SORTES, at Antium went likewise under that denomination. Sig. Havercamp. ubi sup. p. 324, 325.

by the Romans and the Etruscans, will (15) not admit of a doubt. The Romans seem to have used the words SORS and FORTUNA for one deity, on some occasions; and, on others, the term SORTES, as applicable to more divinities, and FORTVNAE, or FORTUNAE ANTIAT, as relative to two, to whom they assigned the (16) epithets FORTIS, and FELIX. One or both of those epithets may possibly be pointed out to us by the letter F, which precedes the words SOR ANT, on the basis below the bust of the goddess SORS, on the reverse of the coin in question. But that this is the true import of the word to which that letter belongs, I must by no means take upon me positively to affirm.

VI.

The medals of the Plætorian family similar to that I have been considering Havercamp (17) takes to have been struck in the time of the civil war, that succeeded Julius Cæsar's death; in which, perhaps, he may not be very remote from truth, though this he has not irrefragably proved. If it should, however, be allowed probable by the learned, the coin before me, which must be nearly of the same date with that war, will seem to have preceded about forty years the birth of CHRIST.

(15) Vid. Anton. Francisc. Gor. ubi sup. p. 214. 215. Sam. Pitisc. in *Lex. Antiquitat. Romanar.* pass. aliosque author. quam plurim.

(16) Fabrett. *Inscript. Antiqu.* cap. ix. p. 632. Sig. Haverc. ubi sup. p. 324, 369. Amstelædami, 1734. Vid. etiam Vulp. *Vet. Lat. Profan.* tom. III. cap. v. p. 98, & seqq.

(17) Sig. Haverc. ubi sup. p. 325.

VII.

VII.

Who P. Cofinius, whose name seems to have been handed down to us by the Denarius I have been attempting here to explain, was, or what was the particular mode of his connection with M. Plætorius, by whom the piece was struck, I cannot at present, for want of sufficient light from antient history, and authentic Roman monuments, take upon me to decide. But this I may be allowed to say, that the piece before me is the only coin of the Cofinian family that has hitherto escaped the ravages of time. That the Cofinian family was of some note in Rome, we may infer, not only from the very curious denarius that is the object of my attention here, but likewise from two or three antient (18) Roman inscriptions, which have preserved to us the name of that family. As for M. Plætorius, mentioned on the denarius before me, (19) and other similar coins, he was, according to M. Havercamp, (20) questor to Brutus, one of Cæsar's murderers; and the piece I am endeavouring to explain first appeared, as already observed, a little (21) after that emperor's death. The Etruscan letters were not then intirely out of use: nay, they were not totally disused in some parts of Italy, and particularly at (22) Falerii, a considerable

(18) Jan. Gruter. *Corp. Inscript. ex recens. Jo. Georg. Græv.* p. CMLXXI. 9. DCLVIII. 1. Amstelædami, 1707. Ludovic. Anton. Murator. *Nov. Thesaur. Vet. Inscript.* p. DCCXCIV. 7. Mediolani, 1740.

(19) Vid. I. Vaill. & Sig. Haverc. in *Fam. Plætor.*

(20) Sig. Haverc. ubi sup. p. 325.

(21) Id. *Ibid.*

(22) Strab. *Geogr. Lib. V.*

number of years after that tragical event. This we learn from Strabo, who flourished when Tiberius sat upon the imperial throne.

VIII.

Having now finished my attempt to elucidate a very curious inedited Etruscan coin, highly meriting the attention of the learned ; I would flatter myself, that an acquisition is hereby made to the science of antient medals, and consequently that this paper may prove not altogether unacceptable to the Royal Society. You will therefore be pleased to lay it before that very learned and most illustrious body ; and believe me to be, with all possible consideration and esteem,

S I R,

Your much obliged,

and very affectionate,

humble Servant,

Christ-Church, Oxon.
Oct. 10, 1771.

John Swinton.

IX. *A Deduction of the Quantity of the Sun's Parallax from the Comparison of the several Observations of the late Transit of Venus, made in Europe, with those made in George Island in the South Seas: Communicated by Mr. Euler, jun. Secretary of the Imperial Academy of Peterburg; in a Letter to Charles Morton, M. D. Sec. R. S. and Acad. Imper. Petrobürg, &c. Soc.*

Read March 5, 1772. **O**BSERVATIONES novissimi transitus Veneris ante discum Solis, A. 1769, in insula maris Australis, King George Island dicta institutas, calculo nuper subjecit academicus noster D. Lexell, earumque comparationem cum observationibus celebratissimi hujus phænomeni in Europa factis instituit, ut quantitatem parallaxis Solaris erueret. Longum omnino foret, si omnium calculorum ab ipso institutorum justam hic adferre vellemus expositionem, idemque etiam minus necessarium, quum dissertatio ejus hac de re conscripta, Tomo XVI. nostrorum Commentariorum inserenda sit; summa igitur tantum capita conclusionum inventarum perstringere suffecerit. In antecessum vero, e re est monuisse, methodum ab ipso adhibitam, eandem esse, quam Illustr. Eulerus invenit et quæ in
part.

part. II. Tom. XIV. nov. Comment. fuscè explicata fuit.

Siquidem observationes contactuum externorum super insula King George institutæ, cum momentis pro contactibus internis allatis, non satis bene consentire videantur, auctor noster duos casus separatim sibi considerandos esse ratus est, priorem quo ratio habetur tam contactuum externorum, quam internorum, alterum vero, quo contactus tantum interni adhibentur. Combinatione autem facta observationum insulæ King George cum iis in Europa institutis, pro priori casu inventa est parallaxis Solis horizontalis $\pi = 8,68 - 0,0077 y$, pro posteriori $\pi = 8,58 - 0,0080 y$; ubi notandum est y significare correctionem latitudinis geocentricæ Veneris pro assumpto tempore conjunctionis. Dum simili ratione observationes in Fortalio Principis Walliæ ad Sinum Hudsonis factæ, cum Europeis comparantur, habetur pro priori casu $\pi = 8,82 - 0,0019 y$ et pro posteriori $\pi = 8,74 - 0,0022 y$. Observationes denique Californienses, cum Europeis comparatæ præbent $\pi = 8,61 - 0,0062 y$. Ut inter has conclusiones medium quoddam ad veritatem proxime accedens eligi posset, notandum omnino fuit, singulis earum eo majorem certitudinis gradum tribui debere, quanto majores fuerint coefficientes, quibus litera π in illis æquationibus affecta deprehenditur, ex quibus valores supra allati hujus quantitatis elicitum sunt; quo major enim hujusmodi coefficientis fuerit, eo sane minorem influxum errores in observando commissi habebunt, ad verum valorem Parallaxis immutandum. Probabilitates igitur conclusionum ex singulis observationibus Americanis deductarum hoc modo æstimando, inventum

inventum est, eas respective habendas esse proportionales numeris 11, 8, et 4. Porro ut observationes majori fide dignæ ab incertioribus secerni possent, auctor noster tres statuit hypotheses; 1. qua modo supra dicto medium sumitur ex conclusionibus quæ inveniuntur, dum omnes sine discrimine observationes in usum vocantur et pro qua habetur $\pi = 8,63 - 0,0063 y$; 2. qua pro insula Regis Georgii momenta solum contactuum internorum in computum ducuntur, unde deducitur $\pi = 8,57 - 0,0057 y$, et 3. denique qua observationes contactuum externorum ad Sinum Hudsonis factæ excluduntur, quæ præbet $\pi = 8,62 - 0,0065 y$. Quum tamen nulla sufficiens adesse videatur ratio cur momenta contactuum externorum ad Sinum Hudsonis pro dubiis haberi deberent, medio quasi sumto, inter media ex binis posterioribus hypothesebus deducta parallaxis tuto statui posse videtur $\pi = 8,60 - 0,006 y$. Ad hujus conclusionis ulteriorem verificationem, singulæ observationes Americanæ comparatæ fuerunt cum iis in Lapponia factis, ubi tam ingressum quam egressum Veneris observare licuit, siquidem pro hujusmodi observationibus, errores qui ex longitudinibus locorum perperam æstimatis in parallaxin redundant fere nullius sunt momenti. Tum autem mediis uti supra dictum est captis, inventi sunt pro singulis tribus hypothesebus sequentes valores ipsius π :

1. $\pi = 8,68 - 0,0076 y$. 2. $\pi = 8,67 - 0,0074 y$.
3. $\pi = 8,62 - 0,0077 y$. Hæ autem conclusiones a supra inventis non magis discrepant, quam ut diversitatis ratio ex leviusculis observationum erroribus facile reddi queat. Ex singulis denique observationibus Americanis inter se collatis sequentes elicitæ sunt valores

valores ipsius π ; ex contactibus externis ad Sinum Hudsonis et super insula King George $\pi = 9,16 - 0,011 y$; ex contactibus internis in iisdem locis $\pi = 8,47 - 0,011 y$; ex contactibus internis ad Sinum Hudsonis et Californiæ $\pi = 8,46 - 0,0096 y$; ex contactibus denique internis Californiæ et super Regis Georgii insula $\pi = 8,48 - 0,0012 y$.

Quod conclusionem ex contactibus externis deductam attinet, facile quidem liquet, eam plus justo a veritate aberrare; quod vero determinationes ex contactibus internis derivatæ, quasi in alteram partem peccent parallaxin supra inventa aliquanto minorem exhibentes, ex erroribus observationum facile admittendis provenire potuit. Ad valorem absolutum ipsius π assignandum, quum jam requiratur, ut vera magnitudo correctionis y innotescat, ea sollicitè quoque determinanda fuit; momenta autem contactuum interhorum in eo consentire videntur, quod hæc correctio circiter $8''$ statui debeat, posita semidiametro Solis $946,38''$, quæ media est, inter semidiametri valorem a Cel. de la Lande assumptum, et quem astronomi Angli adhibere solent. Hoc autem valore pro y adhibito, erit parallaxis $\pi = 8,55$ sec. semidiameter autem Veneris $= 28,6$ sec. quæ ultima determinatio ultra 2 aut 3 partes decimas unius secundi erronea esse nequit, observationibus micrometro objectivo captis eam egregie confirmantibus. Si correctio latitudinis aliquanto esset minor, quod ex observationibus micrometricis redditur probabile, quum tamen infra $5''$ certe deprimi nequeat, parallaxis inde ultra partem quinquagesimam secundi non reddetur dubia.

Elementa autem astronomica ex valore parallaxis deducta jam ita se habebunt: conjunctio vera Solis et Veneris geocentrica contigit 1769 d. 3 Jun.

10^h 2' 32" Temp. medio

Grenovicensi

feu

10 4 45

Temp. vero.

Pro quo momento erat,

1. Longitudo Solis et Veneris 2 13 27 20

2. Latitudo geoc. Veneris 10 18,8 Bor.

3. Parallaxis Solis horizontalis 8,55

Hinc facile elicitur longitudinem geographicam loci super insula King George, ubi observatio peracta fuit, a meridiano Grenovicensi statui debere 9^h 58' 6".

Quum pro insula Regis Georgii ea contactuum momenta, in calculo adhibita fuerint, quæ a Cel. Green assignata deprehenduntur, excepto momento contactus interni pro ingressu, ubi momentum a Cel. Doct. Solander notatum, in usum fuit vocatum, merito dispiciendum erat, quam subeat parallaxis mutationem, si alia contactuum momenta pro hoc loco notata adhibita fuissent. Quod igitur contactus internos attinet, duas supponere licuit hypotheses bina quasi extrema in se continentes, priorem qua observatio contactus interni pro ingressu a Cel. Green facta cum ea contactus interni pro egressu a Cl. Cook instituta combinatur, posteriorem qua mutata vice observatio prioris contactus interni a D^{no} Cook facta combinatur cum momento posterioris contactus interni a Cel. Green assignato. Prior hypothesis dat parallaxin $\pi = 8,48 - 0,0080 y$, posterior vero $\pi = 8,65 - 0,0080 y$, ubi medium $\pi = 8,57 - 0,0080 y$

vix differt a determinatione ante adhibita. De momentis contactuum externorum generatim quidem liquet, ea cum observationibus interiorum nequaquam conciliari posse, quoniam mora inter utriusque generis contactus vera multo minor observata sit; quo certius tamen constaret præcipuam aberrationis causam in contactibus externis esse querendam, sequenti modo in exactitudinem momentorum tam contactibus externis quam internis respondentium, auctori nostro inquirere visum est. Momenta contactuum interiorum, cum observatione ejusdem contactus pro ingressu Grenovici a Celeb. Maskelyne facta comparat, indeque deducit assumptam longitudinem pro King George Island $22''$ vel $25''$ esse augendam: hæc deinde momenta similiter comparando cum observatione contactus interni pro egressu in Gurjes a Cel. Lowits instituta, invenit eandem longitudinem $25''$ vel $20''$ esse augendam, ex quo quum hæc correctio per quatuor diversi generis comparationes fere eadem prodeat, concludendi rationem habere sibi visus est, in contactibus internis graves errores latere nequaquam probabile esse. Dum pro contactibus externis simile instituendum fuit examen, ne videretur parallaxi sinis parvam supposuisse, studio maximam elegit quæ ex his contactibus deduci potest, scilicet $\pi = 9,04 - 0,0073 y$, unde deducit correctionem longitudinis geographicæ pro King George Island ad $32''$ assurgere. Postmodum facta combinatione binorum contactuum externorum, cum observatione contactus interni pro egressu in Wardhus a Rev. Pat. Hell instituta, invenit correctionem longitudinis per unam comparationem prodire 49 sec. per alteram vero 16 , quæ insignis discrepantia certe non nisi ab incertitudine

dine contactuum externorum pro insula King George oriri potest, siquidem longitudo Wardhusii vix ultra 5'' esse possit dubia.

Quoniam plurimis astronomis placuit parallaxin multo majorem adhibere, quam quæ nunc inventa fuit, operæ etiam pretium erat disquirere, quousque errores observationum pro contactibus internis perfringere debeant, si parallaxis supponatur vel 9 vel 10 secundorum. Inventum autem fuit pro parallaxi 9'', si bini contactus interni pro Insula Regis Georgii, cum ejusdem nominis Europeis conferantur, summam errorum ad 40 fere secunda incrementum, pro parallaxi autem 10 sec. eam duobus minutis primis minorem supponi non posse, quorum ut posterius absurdum, ita prius quoque valde improbabile videtur.

Deinde expendendum quoque fuit, utrum ex observationibus distantiarum minimarum, certi quicquam de parallaxi Solis concludi possit. Si autem comparatio instituatur distantie minimæ pro Insula King George inventæ cum distantis ad sinum Hudsonis, Noritoni in Pensylvania, et Novæ Angliæ a Cl. Winthrop mensuratis, inveniuntur pro parallaxi hi valores $\pi = 10'',51$, vel $\pi = 8'',98$, vel $\pi = 9'',97$, quorum dissensus jam certissimo est indicio quam parum fidei hujusmodi observationibus tribui debeat, in quaestione hujus generis decidenda. Cæterum si fuerint, qui ex his mensuris parallaxin 9'' vel etiam 10 sec. deducere velint, ab illis sequentis dubii plenam solutionem merito expectamus: qui fiat, ut potius supponi debeat, singulis observationibus contactuum internorum à tot observatoribus institutis, ad minimum errores 20 sec. vel 30 sec. inesse, quam binas

vel tres mensuras distantiarum minimarum micro-
metro captas 2 aut 3 secundorum erroribus obnoxias
esse? Denique et auctori nostro placuit quædam ad-
jicere de effectu atmosphæræ Veneris ad durationem
transitus imminuendam; observavit autem hanc quæ-
sitionem in dubio relinquendam esse, donec exactissime
determinare licuerit, tam latitudinem geocentricam
Veneris, seu distantiam minimam veram, quam ac-
curatam mensuram diametri Solis.

Præterea reticendum quoque non est, eundem
D. Lexell observationes super distantias marginum
Solis et Veneris minimas, Noritoni institutas, quas
illustr. Societas Scientiarum nobiscum communicavit,
calculo subjecisse, et ex pulcherrimo consensu singula-
rum fere observationum, conclusisse hanc distantiam
minimam fore $10' 10''$ posita semi-diametro Solis
 $947''$, seu etiam $10' 9''$ posita semi-diametro Solis
 $945'',5$ uti celeb. Maskelyne eam assignare solet.
Existimat autem idem D. Lexell utramque distantiam
minimæ quantitatem uno secundo augeri posse, quia
probabiliter videtur distantias marginum justo ma-
jores esse captas, quam quod in defectu peccaverint.

X. *A Letter to the Rev. Mr. Maskelyne, Astronomer Royal, F. R. S. accompanying a new Chart of the Red Sea, with two Draughts of the Roads of Mocha and Judda, and several Observations made during a Voyage on that Sea, by Capt. Charles Newland.*

S I R,

Read March 12, 1772. **I** Beg leave to lay before you a chart of the Red Sea, constructed from materials that I became possessed of, during my residence in the East Indias; which chart, upon my voyage to Mocha and Judda, I experienced to be the best I have ever seen. The only material error that I ever discovered in it, is, that the Abyssinian shore opposite Mocha is placed too far to the westward by 25 or 30 miles, and that there are several small islands upon the same shore, not taken notice of in any other chart; which islands I have marked in the chart I now send you (See Tab. III.) together with two draughts of the roads of Mocha and Judda (Tab. IV.), which, if you think will be of any utility, are entirely at your service.

I am, S I R,

With the utmost respect,

your most obedient humble servant,

Cha^s. Newland.

Longitude

Longitude of Judda by 12 Distances of the ☽ from ☉.

Work'd by the British Mariner's Guide	39° 53' 45"
And by the Ephemeris for 1769	40° 1' 7"

Difference	7 22
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By Jupiter's Satellites	39° 26' 45" E.
By ☽ and ☉	40° 1' 7" E.

34 22

XI. *Remarks and Observations made on board the Ship Kelfall, on a Voyage to Judda and Mocha, in 1769, by the Same.*

Read March 12, 1772. **I**N my run from Socatra to Cape Aden, I made the dist. $8^{\circ} 20'$ W. and from Cochin $29^{\circ} 39'$ W. The latitude of the above Cape is laid down in most books and charts in 13° N. which I find is about 15 miles too much to the northward, according to the observation I took on the 10th of February 1769, as well as three other very good observers: by the medium of the whole we made the latitude $12^{\circ} 41'$ N. the Cape then bore N. N. W. $\frac{1}{2}$ W. dist. 5 miles which gives near $4'$ of latitude; that, added to $12^{\circ} 41'$ N. gives $12^{\circ} 45'$ N. for the latitude of the southernmost point of the Cape.

This cape, or headland, is one of the most remarkable I ever saw, when coming from the eastward; it is so very high and rugged, that it may be seen, I believe, 15 leagues at least, in fine weather. The tops of those ragged rocks resemble so many chimneys and spires; and, as you approach the cape, you see a zigzag wall, or whitish pathway, cut through the rocks, not at a very great distance from the waterside; a little below this, at the S. E. end,

end, you will see something that looks very like two mosques; but this cannot be seen at a greater distance than 4 or 5 leagues; but when it is, you may be certain it is Cape Aden, and may then steer your course for Babelmandel accordingly.

A little to the westward of this cape, there is another high craggy headland, equally high and craggy as that of Aden, between which two there is an opening, very much resembling a small narrow streight, but in reality it is only a deep bay, the bottom of which is very low land, so low, that it cannot be seen from the mast-head, except you are close in shore: by this deception, people have mistaken it for the Streight of Babelmandel, and have been so far embayed, before they perceived their mistake, that it was with the greatest difficulty they got out again.

On each side of this bay lies a large rock, just at the entrance, and at about a quarter of a mile from the shore: when these are seen, you may be sure it is not the Streight of Babelmandel. Was a ship to fall in with this place, and had not had an observation for some days before, I think it would be very easy to mistake one for the other; there is only this difference, that Cape Aden is high and rugged, and Babelmandel is rather low and smooth, and the island (as the Directory observes) makes like a gunner's coin.

The best course to steer from Cape Aden to Saint Anthony is W. by S. by the compass, and that will carry you clear of the shoal lying off that point. I made the distance between Cape Aden and Cape Saint Anthony, by the ship's run, 17 leagues; the latter

Fig. 1.

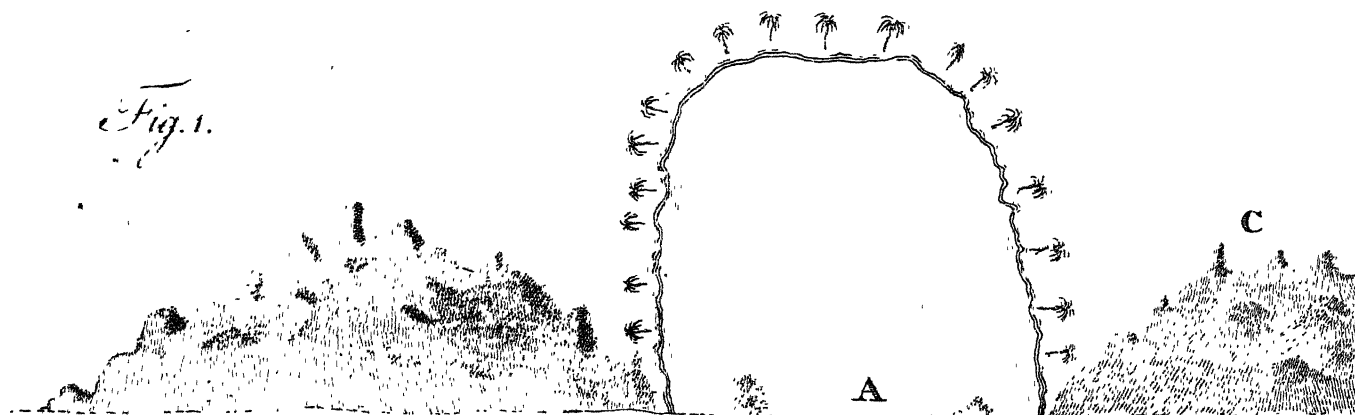


Fig. 5.



Fig. 3.

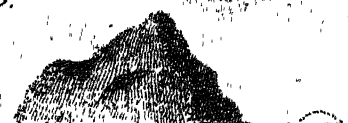


Fig. 2.



Fig. 4.

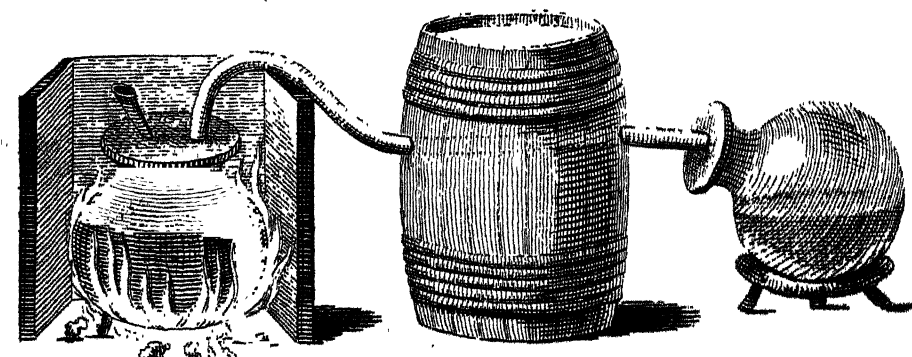


Fig. 6.

cape is high land, and may be seen in fair weather about 12 leagues.

N. B. When Cape Aden bore N. N. W. $\frac{1}{2}$ W. about 5 miles, I had 40 fathom, coarse sand and small shells, the opening of the small bay appeared like the narrow Streight of Babelmandel, N. W.

Thus sheweth Cape Aden C, when the opening A bears W. N. W. $\frac{1}{2}$ N. distance about 4 miles.

Tab. V. Fig. 1.

Thus sheweth Cape Aden, when coming from the eastward, at the distance of about 7 leagues, and when it bears about W. by N. Fig. 2.

To give any directions for sailing through the narrow Streight of Babelmandel, and from thence to Mocha road, would be needless, as they are so extremely good in the East India Directory, as also for anchoring and sailing into the road, with proper bearings, and distance to anchor from the town.

Thus sheweth Cape Babelmandel, when it bears N. W. by W. distance 6 or 7 leagues, Fig. 3.

Thus sheweth Cape Aden, when it bears W. by N. 10 or 11 leagues, Fig. 4.

From Mocha towards Judda, the islands of Jebbel-Zeker Aloric are pretty large, and may be seen in clear weather 7 or 8 leagues; they are six in number, the southernmost lies in the latitude of $13^{\circ} 45'$ N. and bears from Mocha N. W. by W. nearly, distance

about 40 miles. A little to the northward of those islands lies Jebbel-Zeker, a very high large island, that may be seen in fair weather 12 or 13 leagues. Very near this island, of the N. E. side, lie three small ones, not discernible at a distance of 4 leagues. The N. end of the large island Jebbel-Zeker lies in the latitude of $14^{\circ} 10' N$.

In coasting along the Arabian shore, abreast of the large island, care must be taken not to come too near the shore, as there is a shoal water, between the mosque of Cape Name and Cape Namel, 7 or 8 miles from the shore, and foul ground, with overfalls. The edge of this bank is very steep too; for when I was from the shore about the above distance, I had presently from 20 to 7 fathom water, and then 6 fathom. I immediately hauled off, and deepened my water again very soon, to 13, 14, and then 20 fathoms, as may be seen by the soundings in the draught, Tab. IV.

The true course from Jebbel-Zeker to the Suburgars is N. W. by N.; distance 20 leagues. Those islands are extremely well laid down in three different charts I have met with for the Red Sea; they trench away about N. N. W. and S. S. E. and extend from N. to S. about 20 miles; they are nine in number, and not very high, however, I believe they may be seen, in clear weather, from the mast-head 7 or 8 leagues; the latitude of the S. and N. ends $14^{\circ} 57'$ to $15^{\circ} 15' N$.

N. E. of those islands lies a low white island (which I call Sandy Island), environed all round with shoal water; to the southward of which, the shoal seemed, from the mast-head, to extend from 2 or 4 miles. I passed it at about 6 miles distance,

distance, and never had less than 26 fathom, sandy ground. Two or 3 miles within me, appeared like very shoal water. Its latitude is $15^{\circ} 22' N.$

About 40 miles N. N. E. from the Subugars, lies the Island Comoran, a very low blackish island, trenching away N.W. and S.S.E. excepting the north end, which turns off suddenly, and stretches away N. N. E. When at the distance of about 5 miles from this island, I had 23 fathom water, hard sand and gravel stones, at the same time it bore E. N. E. and, when the body bore N. E. distance about 6 miles, I saw a large square white house near the waterside.

N. W. by N. by the compass, from the Subugars, lies the Island Jebbel-Tar, distance about 25 miles. This island is of a moderate height, and may be seen 9 or 10 leagues from the mast-head, in clear weather; its latitude is about $15^{\circ} 36' N.$ and when it bore W. about 10 miles, I had 33 fathom water, a sandy bottom.

After taking your departure from this island, when bound to Judda, the best course to steer is N. N. W. $\frac{1}{2}$ W. which is near the mid channel; by so doing, you may run boldly on all night, without fear.

From Jebbel-Tar to the small islands on the Arabian side, laid down in about $18^{\circ} N.$ latitude, I made the course N. $22^{\circ} 49'$ W. distance 159 miles. It was about sun-set when I first saw two of those islands; they then bore from N. E. to S. E. by S. distance from the nearest of them about 6 miles, and breakers a little to the southward about 5 miles. The southernmost of these islands lies in the latitude of $18^{\circ} 2' N.$ according to my bearing and distance at

sun-set; they are very low, but long, and stretch to the northward; I had no soundings, 80 fathom at the above distance.

From the above islands I saw no dangers, till I was in the latitude of $19^{\circ} 24'$ N. (upon the Ethiopian shore), when I saw breakers; and a little to the N. W. of them I saw a low sandy island, that cannot possibly be seen at a greater distance than 6 or 7 miles; at the same time I saw two high islands to the west and northward of them, distance 8 or 10 leagues.

And, upon the eastern or Arabian shore, in the latitude of $20^{\circ} 14'$ N. I saw a low small sandy island; and 3 or 4 miles farther to the northward, another low sandy island, about the size of the former, neither of which can be seen, in clear weather, at a greater distance than 3 leagues. At the same time, a little to the northward and westward withal, I saw breakers very plain from the ship's deck; those islands were called (by the pilot I had on board) Marfaharam; he at the same time informed me, that it was very dangerous to go to the eastward of them, it being nothing but shoals and rocks. When you are to the northward of these islands and breakers, you will see the high land of Goofs, at the distance of 14 leagues, the approaching of which is very dangerous without a pilot, as well as all the rest of the coast quite to Judda, it being so incumbered with rocks and shoals; and what makes it the more hazardous is, there being no soundings till you come at once upon a hard steep sand-bank, or a ledge of rocks; therefore, it will be absolutely necessary to take a pilot on board, in or about 20° N. latitude, if possible, but should you not be so fortunate

fortunate as to get one before you come near Judda, it would be most certainly prudent to keep 30 or 40 miles from the shore, at least so far that you can but just discern the high land of Goofs and Gedan, at which distance there is no danger. Although this may appear a great distance for the pilots to come off to the ship, yet they will immediately do it as soon as they hear your gun, and not till then.

It is indeed amazing, and almost incredible to be told, how far these pilots will hear the guns on a still morning or evening, which are the proper times for the guns to be fired. Observe to fire the first as soon as you see the sun appear in the horizon, and the second as soon as the lower limb is just out of the water; in the evening, the first as soon as the lower limb touches the water, and the second when the upper limb is below the horizon. Four firings in one day is all that are necessary; but they are to be repeated every day till you get a pilot. They know pretty near the time the India ships will arrive, and go down to the water side every night and morning, and just as the sun is rising or setting, they lay their ear close to the ground for three or four minutes, and pretend to say, that if a ship is not more than two or two and a half degrees distance when the gun is fired, they can either hear the report or find the ground shake under them; upon which they take a boat and come off to pilot you in. This may seem a little extraordinary to a person that never was there; but, however strange it may appear, I was assured by a gentleman of undoubted veracity, that he run by the log 95 miles from the time of firing his two guns in the morning, till he saw the pilot in the evening; and when he came.

came on board, he declared that he heard the two guns that morning at sun-rising, upon the strength of which, he took his boat and put off.

To sail into Judda harbour, or rather road, without a pilot, would be impossible for a stranger, there being so many sand-banks and shelves of rocks; but when you are in, it is one of the safest places that can possibly be; you may make your ship fast with any old junk, and there is no danger, though you are surrounded with nothing but rocks and sands.

The best bearing for anchoring is the great Mosque E. by S. and the extremes of the land from S. by E. to N. N. W. distance from the landing-place about two miles.

Latitude of Judda	21° 28' " N.
Longitude Ditto	39° 26' 45" E.
Variation of the compass	11° 52' W.

I left Judda the 30th of July 1769, and passed by the grap shoals (lying in the latitude of 21° 20' N.) at about a mile's distance, from which I took my departure and made my course the first day S. 24° W. dist. 56 miles; the second day, S. 15° E. dist. 100 miles. On the third day about 6 o'clock in the afternoon I saw a very high land on the Ethiopian shore (about the latitude of 18° 38' N.) and some small islands a little to the Northward of it; the high land bore about W. N. W. and the small islands N. W. dist. from the high land about 10 leagues; we had then run from the Grab shoal 195 miles.

From

From the ship's place at 6 P. M. run about 68 min. S. S. E. by the compass, and then made a low long sandy island bearing S. W. about 8 miles on the same shore, on the South end of which are breakers that may be seen 8 or 9 miles; the middle of this island lies in or near the latitude of $16^{\circ} 42'$ N. from thence to Jebbel-Tar the distance is 128 miles, in a direct S. E. course by the compass.

Thus sheweth the island of Jebbel-Tar, when it bears W. by S. dist. about 10 miles; Tab. V. fig. 5.

From Jebbel-Tar to Jebbel-Zeker, the course is by the compass S. E. by S. distance about 100 miles: the passage to the Westward of it I had been informed was a very good one, but I find by experience it is not so good as that to the Eastward of it; nor do I think it so safe to go that passage in the night, except you are close to the island before it is dark, and well to the Southward of the large Jebbel-Zeker, so that you can see the Southernmost of the small Jebbel-Zeker Alories, as I found a very strong current setting upon the Abyssinian shore, and to the Southward withal. On the evening of the 4th of August at sun-set, the large Jebbel-Zeker bore E. by S. distance about 4 leagues, and the Southernmost one S. S. E. half E. Having a very fine wind, and wanting to be at Mocha very much, I carried a moderate sail, and steered from sun-set till 8 o'clock South about 7 miles, from 8 till 10 S. by E. 7 miles, from 10 till 12 S. S. E. 7 miles, and then bore away, concluding myself well to the Southward of all the islands of Jebbel-Zeker Alorie; and indeed had my draught been good, and had I met with no current, I should have been clear of every thing according to my

my run, which gave me about 5 miles to the Southward of the Southernmost Jebbel-Zeker; but to my great surprize, just as I was bearing away, I saw two islands right a-head, within about half a mile of us. We were then going at the rate of 4 knots: I immediately hauled off to the S. W. and soon after S. till I thought we were quite clear, and then (about two o'clock) going to bear away a second time, we discovered two very small islands, less than a mile from us, right a-head, upon which I shortened sail, and laid the ship's head to the Westward till day-light, when I perceived myself surrounded with a parcel of small islands (not laid down in any chart), about half way between Jebbel-Zeker Alories and the Abyssinian shore. It falling little wind, and the current driving me very fast upon the Abyssinian shore, I let go my anchor in 27 fathom sandy bottom, and there laid till 10 in the morning, when a breeze of wind sprung up from the Northward; I then immediately hove up my anchor, and stood over for Mocha steering N. 77° E. distance 39 miles: when at anchor, I was about 3 miles from the Abyssinian shore, and about half a mile from a large rock, or rather a small island. While I was among these islands, I saw no breakers or shoal water; the least water we had was 27 fathom, and never more than 40 fathom, and mostly sandy ground. Most of these small islands I have marked in my chart, pretty near as they bear from each other; the distance from the Abyssinian to the Arabian shore is not more than 40 or 45 miles (about the latitude of $13^{\circ} 25' N.$) though in most charts it is made to measure from 70 to 80 miles, which certainly must be a great deal too

too much; for, supposing an error in my run from shore to shore 10 miles, it would only make the distance 50 miles, which is 25 or 30 miles less than the charts give.

Latitude observed at Mocha	13° 23' N.
Variation of the compass	12° 33' W.

XII. An easy Method to distill fresh Water from Salt Water at Sea ; by Capt. Newland.

Read March 12, 1772. **T**HE materials necessary for this process are the following ; a copper or iron pot of 15 or 20 gallons, an empty cask, some sheet lead, a small jar, a few wood-ashes or soap, and billet-wood for fuel. See Tab. V. fig. 6. where A is the still or pot ; B the pipe or worm ; C the worm-tub ; D the receiver ; EE the fagong or fire-place ; and f the plug-hole to put in water.

FIRST, In order to make my pipe or worm B, I took as much sheet lead as I thought was sufficient for the purpose, and beat it on a sponge staff to make it round : this being done, I was somewhat at a loss for folder ; however, I supplied that deficiency with good paste and dungerec (or thin canvas) laid well on, and over that, a second coat of paste and dungerec, and then a covering of small cored line hove close together and very tight round, over which I put my third coat of paste and dungerec, which I found, to my great satisfaction, was sufficient to keep it from blowing. The next thing was to fix my pipe B in the pot or still head A. When I had well secured the pot in the fagong EE. I filled it about two thirds full of salt water (about 15 gallons), with which I mixt two or three double handfulls of wood-ashes, and stirred it well together, in order to soften the salt water ; I then fixt the lid (which was made

made of plank 3 inches thick) in which there are two holes, one for the end of the pipe, the other to put in water as occasion requires, without taking off the lid. It must be well observed, that the end of the pipe is not put more than 2 or 3 inches within the still head; for, should it be put too far in, when the water boils, the bubbles or saline particles get into the end of the pipe, and make the water brackish in the receiver D. To prevent the steam from coming out at the plug-hole *f* or lid A, I made a kind of mortar, with wood-ashes, salt water, and rope cut very small and beat well together, and then applied it thereto, which answered my purpose extremely well. Now my pipe is fixt in the still-head, I shall proceed in the next place to carry it through the worm tub C into the receiver D. My worm-tub is nothing more than an empty cask with one of the heads taken out, and in each side a round hole cut, of about 3 inches diameter, for the pipe B to pass through into the receiver D, which is fixt at a little distance from the tub C. The receiver has also a wooden lid like that of the still-head, with a hole in it for the end of the worm to go through into the receiver D; care must be taken, that no steam comes out there, as well as at the still-head. An empty jar will answer the purpose of a receiver very well. Notwithstanding the pipe B passes through the tub C of cold water, your jar will be very hot; I therefore thought it necessary to keep a person continually wetting it with cold water, which not only kept the jar from breaking, but made the fresh water cold and fit for use immediately after the still was

taken off. The foregoing directions strictly observed, a quantity of 8 or 10 gallons will be produced every day, each day containing 12 hours.

Note. Every five or six hours you must replenish the still with about five gallons of water, as I found my first stock consumed about a gallon per hour by boiling.

XIII. *Observations on the milky Appearance of some Spots of Water in the Sea; by the Same.*

Read March 12, 1772. **I**T has been remarked by several navigators, on their passage from Mocha to Bombay, Surat, &c. that they had discovered in the night spots of water as white as milk, and could never assign any reason for it; and many have been so much alarmed, that they have immediately hove to and founded; but I never heard of any body ever getting ground. In my passage across those seas in the Kelsall, I discovered all of a sudden, about 8 o'clock in the evening, the water all round me as white as milk (intermixt with streaks or serpentine lines of black water). I immediately drew a bucket of it, and carried it to the light, where it appeared just as other water; I drew several more, and found it the same: some I kept till the next morning, when I could perceive no difference from that alongside. We had run by the log 50 min. from the time we first observed it till daylight, and during all that time the water continued white as milk, but at full daylight it was of its usual colour. The next evening about 7 o'clock the water appeared again as white as before; I then drew another bucket and carried it to a very dark place, and holding my head close to the bucket could perceive,

ceive, with my naked eye, an innumerable quantity of animalcules floating about alive, which enlightened that small body of water to an amazing degree. From thence I conclude that the whole mass of water must be filled with this small fish spawn or animalcules, and that this is without all doubt the reason of the water's appearing so white in the night-time. We run by the log, from the time we first saw it till the latter part of the second night (the time we lost sight of it) about 170 miles.

N. B. Latitude about $15^{\circ} 10'$ N. and S. W. dist. from Cape Aden $12^{\circ} 18'$ E.

On the 30th of August 1769, at 3 o'clock in the morning, I saw a comet $8^{\circ} 20'$ from Aldebaran S. W. and the tail streaming to the Westward.

I made the meridian distance from Cape Aden to Striking Sounding on the Malabar coast (in the lat. of $14^{\circ} 2'$ N.) $27^{\circ} 31'$ E.

XIV. *A Letter from Mr. Peter Dollond, to Nevil Maskelyne, F. R. S. and Astronomer Royal; describing some Additions and Alterations made to Hadley's Quadrant, to render it more serviceable at Sea.*

Reverend Sir,

Read March 29, 1772. **T**HE particular attention, you have always shown to any improvement tending to the advantage of astronomy or navigation, makes me take the liberty to trouble you with an account of some additions and alterations I have lately made to the Hadley's quadrant.

The general use of this instrument at sea is so well known, that no mention need be made of the importance of any improvements in the construction, that may render the observation more exact, and occasion more frequent opportunities of making them.

The glasses of the Hadley's quadrant should have their two surfaces perfect planes, and perfectly parallel to each other. From several years practice in grinding these glasses, I have found out methods of making them to great exactness; but the advantage, that should arise from the goodness of the glasses,
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has oftentimes been defeated by the index glass being bent by the brass frame that contains it: to prevent this, I have contrived the frame, so that the glass lies on three points, and the part that presses against the front of the glass has also three points exactly opposite to the former. These points are made to confine the glass by three screws at the back, that act exactly opposite to the points between which the glass is placed. This little contrivance may be of some use; but the principal improvements are in the methods of adjusting the glasses, particularly for the back observation.

The method hitherto practised for adjusting that part of the instrument, by means of the opposite horizons at sea, has been attended with so many difficulties that it has scarce ever been used; for so little dependance could be made on the observations taken this way, that the best Hadley's sextants made for the purposes of observing the distances of the Moon from the Sun or fixed stars, have been always made without the horizon glass for the back observation; for want of which, many valuable observations of the Sun and Moon have been lost, when their distance has exceeded 120 degrees.

To make the adjustment of the back observation easy and exact, I have applied an index to the back horizon glass, by which it may be moved into a parallel position to the index glass, in order to give it the two adjustments, in the same manner as the fore horizon glass is adjusted. Then, by moving the index to which the back horizon glass is fixed, exactly 90 degrees (which is known by the divisions made for that purpose) the glass will be thereby set

at right angles to the index glass, and consequently will be properly adjusted for use, and the observations may be made with the same accuracy by this, as by the fore observation.

To adjust the horizon glasses in the perpendicular position to the plane of the instrument, I have contrived to move each of them by a single screw, that goes through the frame of the quadrant, and is turned by means of a milled head at the back, which may be done by the observer while he is looking at the object.

To these improvements, Sir, I have added your method of placing darkening glasses behind the horizon glasses, which you have been so kind as to give me liberty to apply to my instruments. These glasses, which serve for darkening the object seen by direct vision, in adjusting the instrument by the Sun or Moon, I have placed in such a manner as to be turned behind the fore horizon glass, or behind the back horizon glass, that they may be used with either; there are three of these glasses of different degrees of darkness; the lightest or palest I do imagine will be of use in taking the Sun's altitude when the horizon appears glaring, which I believe often happens by the reflection of the sea.

If these additions and alterations should be thought to be real improvements, which I cannot doubt, Sir, if they are honoured with your approbation, I hope they may serve in conjunction with those improvements you have made yourself in respect to the obviating any possible errors in the parallelism of the planes of the index glass, and in regard to the adjustment of the telescope parallel to the plane of the

quadrant, to extend the use of this most valuable nautical instrument, and to add to the exactness of the celestial observations taken with it to determine the longitude at sea. But of these particulars I need say no more, since you are, without doubt, in every respect, the properest person to give an account of them.

I am, S I R,

Your most obedient,

humble servant,

London, February 25,
1772.

Peter Dollond.

Received May 22, 1772.

XV. *Remarks on the Hadley's Quadrant, tending principally to remove the Difficulties which have hitherto attended the Use of the Back-observation, and to obviate the Errors that might arise from a Want of Parallelism in the two Surfaces of the Index-Glass. By Nevil Maskelyne, F. R. S. Astronomer Royal.*

Read May 28, 1772. **T**HE back observation with Hadley's quadrant being founded on the same principles, and in theory, equally perfect with the fore-observation, and being at the same time necessary to extend the use of the instrument up to 180 degrees (it being impracticable to measure angles with any convenience beyond 120 degrees with the fore-observation) it may seem surprizing that it hath not been brought equally into general use, more especially since the method of finding the longitude by observations of the Moon, has been practised at sea for some years past; since this method would receive considerable advantage from the use of the back-observation in taking distances of the Sun and Moon between the first and last quarter, could such

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observations be as much depended upon as the fore-observations. The causes of this seem to have been principally these two, the difficulty of adjusting the back horizon glass, and the want of a method of directing the sight parallel to the plane of the quadrant. The back horizon glass, like the fore-one, requires two adjustments: the first, or common one, disposes it at right angles to the index glass, when the index stands at (0) upon the arch; which is usually performed by setting (0) of the index of the arch of the quadrant by double the dip of the horizon of the sea, and then holding the quadrant vertical with the arch downwards, and turning the back-horizon glass about, by means of its lever or perpetual screw, till the reflected back horizon appears to coincide with the fore-horizon seen directly. But this operation is so difficult in practice with the back-horizon glass wholly silvered, except a small transparent slit in the middle, as it has been usually made, that few (if any) persons have ever received proper satisfaction from it. If the back-horizon-glass was silvered in every respect like the fore-horizon-glass (which it ought to be) the upper part being left unsilvered, and a telescope was applied to it, perhaps this adjustment might be rendered somewhat easier and more exact; but it could not even thus be made so exact as the adjustment of the fore-horizon-glass may, by making use of the Sun's limbs.

The second adjustment of the back-horizon-glass, in the common construction of the quadrant, is still more troublesome, since it cannot be executed without setting the index 90 degrees off the arch, in order to place the index glass parallel

allel to the back-horizon-glass; when this adjustment may be performed in the same manner as the corresponding adjustment of the fore-horizon-glass. But the bending of the index, that follows the setting it off the arch, is a very disagreeable circumstance, having a tendency, especially on board of ship, to expose both the index and centre work to damage; and may even, without extraordinary precautions taken by the instrument maker in placing the plane of the index-glass exactly according to the length of the index, disturb its perpendicularity to the plane of the quadrant: on these accounts it would be much better if this adjustment of the back-horizon-glass could be performed, like those of the fore-horizon-glass, with the index remaining upon the arch of the quadrant. Fortunately, this *desideratum* has been lately effected by an ingenious contrivance invented by Mr. Dollond, which he has given an account of in a letter addressed to me*, which I have presented to this Society, by means of an additional index applied to the back-horizon-glass; whereby both the adjustments may be made by the same observations and with nearly the same exactness as those of the fore-horizon-glass: for a farther knowledge of which, see the account itself.

Besides the difficulty of adjusting the back-horizon-glass, the want of a method of directing the line of sight parallel to the plane of the quadrant has proved also a considerable obstacle to the use of the back-observation: this will easily appear from the following proposition, that the error of the angle measured arising from any small de-

* See the XIV th paper, which immediately precedes this.

viation of the visual ray from a parallelism to the plane of the quadrant, is to twice an arch equal to the versé-sine of the deviation; as the tangent of half the angle measured by the quadrant is to radius, very nearly. Thus a deviation of 1° in the line of sight, will produce an error of about 1' in measuring an angle of 90° , whether by the fore or back observation; but the same deviation will produce an error of 4' in measuring an angle of 150° , of 6' in taking an angle of 160° , and 12' in taking an angle of 170° . Hence a pretty exact adjustment of the line of sight, or axis of the telescope, is requisite in measuring large angles, such as those are taken by the back observation: and therefore a director of the sight ought by no means to be omitted in the construction of the instrument (as it commonly has been since Mr. Hadley's time, though recommended by him), except a telescope be made use of, which if rightly placed answers the same purpose better, especially in observing the distance of the Moon from the Sun between the first and last quarter. The director of the sight may be placed exact enough by construction; but the telescope cannot, and Mr. Hadley, not having been aware of the importance of an exact position of it, has accordingly given no directions for the placing it. I shall therefore endeavour to supply this defect in the following remarks.

In the first place, I would by all means recommend an adjusting piece to be applied to the telescope, whereby its axis may be brought parallel to the plane of the quadrant: in the next place, the back-horizon-glass ought to be silvered in the same manner as the fore-horizon-glass: and thirdly, two thick silver wires should be placed within the eye-tube

in the focus of the eye-glass parallel to one another and to the plane of the quadrant. If they were put at such a distance as to divide the diameter of the field of view into three equal parts, it might be as convenient as any other interval. In this manner wires were placed in the telescope by Mr. Hadley, as appears by his account of the instrument in *Philos. Trans.* N° 420. These wires are to be adjusted parallel to the plane of the quadrant, by turning the eye-tube round about which contains the wires, till they appear parallel to the plane of the quadrant. The axis of the telescope, by which is meant the line joining the centre of the object-glass and the middle point between the two wires, is to be adjusted parallel to the plane of the quadrant by either of the two following methods.

1st method. When the distance of the Moon from the Sun is greater than 90 degrees, by giving a sweep with the quadrant and moving the index, bring the nearest limbs to touch one another at the wire nearest the plane of the quadrant. Then, the index remaining unmoved, make the like observation at the wire farthest from the plane of the quadrant; and note whether the nearest limbs are in contact as they were at the other wire: if they are, the axis of the telescope is parallel to the plane of the quadrant: but if they are not, it is inclined to the same, and must be corrected as follows. If the nearest limbs of the Sun and Moon seem to lap over one another at the wire farthest from the plane of the quadrant, the object end of the telescope is inclined from the plane of the quadrant, and must be altered by the adjustment made for that purpose: but, if the nearest limbs

limbs of the Sun and Moon do not come to touch one another at the wire farthest from the plane of the quadrant, the object end of the telescope is inclined towards the plane of the quadrant, and must be altered by the adjustment accordingly. Let these operations be repeated until the observation is the same at both the parallel wires, and the axis of the telescope will be adjusted parallel to the plane of the quadrant. In like manner, the axis of the telescope may be also adjusted parallel to the plane of the quadrant for the fore-observation.

Second method. Set the index to (○) and hold the plane of the quadrant parallel to the horizon of the sea, with the divided arch upwards, the two wires being parallel to, and including both the direct fore-horizon, and the reflected back-horizon, between them. Raise or lower the plane of the quadrant until the direct and reflected horizons coincide together: if the coincidence happens in the middle between the two wires, or rather, to be more exact, above the middle by such a part of the field of view as answers to the number of minutes in the depression of the horizon (which may be easily estimated if the angular interval of the wires be first found by experiment, in manner hereafter mentioned) the axis of the telescope is parallel to the plane of the quadrant; but if it does not, the line of sight is inclined to the plane of the quadrant, and must be corrected as follows. If the direct and reflected horizons, when they coincide, appear higher above the middle between the wires, than what the quantity of the depression of the horizon amounts to, the object end of the telescope is inclined from the plane of the quadrant, and must be altered by the adjustment made for that purpose; but

but if the two horizons appear to coincide in a lower part of the field of the telescope, the object end of the telescope is inclined towards the plane of the quadrant, and must be altered by the adjustment accordingly. Repeat these operations till the two horizons appear to coincide above the middle between the two wires, by the quantity of the depression of the horizon, and the axis of the telescope will be adjusted parallel to the plane of the quadrant. In order to find the angular interval between the wires, hold the quadrant perpendicular to the horizon, as in observing altitudes; and turn about the eye-tube with the wires until they are parallel to, and include, the direct fore-horizon and reflected back-horizon between them. Move the index from (o) along the divided arch, at the same time raising or lowering the telescope by the motion of the quadrant until the direct horizon appears to coincide with the upper wire, and the reflected back-horizon with the lower wire; the number of degrees and minutes shewn upon the arch, increased by double the depression of the horizon, will be the angular interval of the wires; its proportion to the depression of the horizon will be therefore known; and hence the space in the field of the telescope answering to the depression of the horizon, may be easily estimated near enough for adjusting the axis of the telescope in the manner before-mentioned. The first of the two methods here given for adjusting the position of the telescope will probably be found most convenient; and the greater the distance of the Sun and Moon is, the more nearly may the adjustment be made, because the same deviation of the axis of the telescope will cause a greater error.

The telescope should be fixed by the instrument-maker so as to command a full field of view when the instrument is placed at 90° if the instrument be an octant, or 120° if it be a sextant; because the index-glass then stands more oblique with respect to the incident and reflected rays, and consequently the field of view of the telescope, as far as it depends upon the index-glass, will be more contracted than in any other position of the index: but if there is a fair field of view in this case, there necessarily must be so in every other position of the index.

The two parallel wires will be very useful on many occasions, as well in the fore as the back observation. In taking the altitude of the Sun, Moon, or star, direct the sight towards the part of the horizon underneath, or opposite to the object, according as you intend to observe by the fore or back observation, and hold the quadrant that the wires may constantly appear perpendicular to the horizon, and move the index till you see the object come down towards the horizon in the fore-observation, or up to it in the back-observation, and turn the instrument in order to bring the object between the wires; then move the index till the Sun or Moon's limb, or the star touch the horizon. The nearer the object is brought to an imaginary line in the middle between the wires (it is indifferent what part of the line it is brought to) and the truer the wires are kept perpendicular to the horizon, the more exact will the observation be. In the fore-observation, the object appears in its real position; but in the back-observation, the object being brought through the zenith to the horizon, the real upper-limb will appear the lowest; and

and the contrary. Either limb of the Sun may be used in either observation; but it will be most convenient in general to make the Sun appear against the sky, and not against the sea; and then the objects appearing inverted through the telescope, the Sun will appear lowest, and the horizon highest. The observed altitude is to be corrected for dip, refraction, and Sun's semi-diameter, as usual.

In taking the distance of the nearest limbs of the Sun and Moon, whether by the fore or back-observation, having first set the index to the distance nearly, by the help of the Nautical Almanac, and brought the Moon to appear anywhere on or near the diameter of the field of view of the telescope, which bisects the interval between the wires, give a sweep with the quadrant, and the Sun and Moon will pass by one another; if in this motion the nearest limbs, at their nearest approach, just come to touch one another, without lapping over, on or near any part of the diameter of the field of the telescope which bisects the interval between the wires, the index is rightly set; but if the nearest limbs either do not come to meet, or lap over one another, alter the index, and repeat the observation till the nearest limbs come to touch one another properly. This method of observing will be found much more easy and expeditious than without the wires, since in that case it would be necessary to make the limbs touch very near the centre of the telescope, but here it is only necessary to make them do so anywhere on or near the diameter of the field of the telescope which bisects the interval between the two wires.

The same method may be used in taking the Moon's distance from a fixed star.

It may not be amiss here to make some remarks on the rules that have been usually given for observing the Sun's altitude, both with the fore and back-observation, which have all been defective, and to point out the proper directions to be followed, when a telescope is not used with two parallel wires to direct the quadrant perpendicular to the horizon, and to shew the principles on which these directions are founded.

Observers are commonly told, that in making the fore observation they should move the index to bring the Sun down to the part of the horizon directly beneath him, and turn the quadrant about upon the axis of vision; and when the Sun touches the horizon at the lowest part of the arch described by him the quadrant will shew the altitude above the visible horizon. I allow that this rule would be true, if a person could by sight certainly know the part of the horizon exactly beneath the Sun; but, as this is impossible, the precept is incomplete. Moreover, in taking the Sun's altitude in or near the zenith, this rule entirely fails, and the best observers advise to hold the quadrant vertical, and turn one's self about upon the heel, stopping when the Sun glides along the horizon without cutting it: and it is certain that this is a good rule in this case, and capable with care of answering the intended purpose. We have thus two rules for the same thing, which is a proof that neither of them is an universal one, or sufficient in all cases alone.

In taking the back-observation, observers have been advised either to turn the quadrant about upon the axis of vision, or, holding the quadrant upright, to turn themselves about upon the heel, indifferently. The true state of the case is this; that, in taking the Sun's altitude, whether by the fore or back-observation, these two methods must be combined together; that is to say, the observer must turn the quadrant about upon the axis of vision, and at the same time turn himself about upon his heel, so as to keep the Sun always in that part of the horizon-glass which is at the same distance as the eye from the plane of the quadrant: for, unless the caution of observing the objects in the proper part of the horizon-glass be attended to, it is evident the angles measured cannot be true ones. In this way the reflected Sun will describe an arch of a parallel circle round the true Sun, whose convex side will be downwards in the fore-observation, and upwards in the back-observation, and consequently, when, by moving the index, the lowest point of the arch in the fore-observation, or the uppermost point of the arch in the back-observation, is made to touch the horizon, the quadrant will stand in a vertical plane, and the altitude above the visible horizon will be properly observed.

The reason of these operations may be thus explained: the image of the Sun being always kept in the axis of vision, the index will always shew on the quadrant the distance between the Sun and any object seen directly which its image appears to touch; therefore, as long as the index remains unmoved, the image of the Sun will describe an arch everywhere equidistant from the Sun in the heavens, and consequently

quently a parallel circle about the Sun, as a pole; such a translation of the Sun's image can only be produced by the quadrant being turned about upon a line drawn from the eye to the Sun, as an axis; a motion of rotation upon this line may be resolved into two, one upon the axis of vision, and the other upon a line on the quadrant perpendicular to the axis of vision; and consequently a proper combination of these two motions will keep the image of the Sun constantly in the axis of vision, and cause both jointly to run over a parallel circle about the Sun in the heavens; but when the quadrant is vertical a line thereon perpendicular to the axis of vision becomes a vertical axis; and, as a small motion of the quadrant is all that is wanted, it will never differ much in practice from a vertical axis; therefore the observer, by properly combining and proportioning two motions, one of the quadrant upon the axis of vision, and the other of himself upon his heel, keeping himself upright (which gives the quadrant a motion upon a vertical axis) will cause the image of the Sun to describe a small arch of a parallel circle about the Sun in the heavens, without departing considerably from the axis of vision.

If it should be asked, why the observer should be directed to perform two motions rather than the single one equivalent to them on a line drawn from the eye to the Sun as an axis? I answer, that we are not capable, while looking towards the horizon, of judging how to turn the quadrant about upon the elevated line going to the Sun as an axis, by any other means than by combining the two motions above-mentioned, so as to keep the Sun's image al-

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ways in the proper part of the horizon-glass. When the Sun is near the horizon, the line going from the eye to the Sun will not be far removed from the axis of vision; and consequently the principal motion of the quadrant will be performed on the axis of vision, and the part of the motion made on the vertical axis will be but small. On the contrary, when the Sun is near the zenith, the line going to the Sun is not far removed from a vertical line, and consequently the principal motion of the quadrant will be performed on a vertical axis, by the observer's turning himself about, and the part of the motion made on the axis of vision will be but small. In intermediate altitudes of the Sun, the motions of the quadrant on the axis of vision and on a vertical axis will be more equally divided. Hence appears the reason of the method used by the best observers in taking the Sun's altitude when near the zenith by holding the quadrant vertical and turning about upon the heel, and the defects of the rules that have been commonly given for observing altitudes in other cases.

As it may conduce to the setting this matter in a still clearer light, I shall here describe in order the several motions that will be given to the reflected image, by turning the quadrant about upon the axis of vision, a vertical axis, or the line drawn from the eye to the Sun, successively.

- I. If the quadrant is turned about upon the axis of vision, the same being directed to the point of the horizon exactly beneath or opposite the Sun, the image of the Sun will move from right to left, or from left to right, across the
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the horizon-glass, the same way as the arch of the quadrant is carried, both in the fore and back-observations, with a velocity which is to the angular velocity of the quadrant as the sine of the Sun's altitude to the radius, describing an arch convex downwards in both cases; and when the motion of the Sun in this arch is parallel to the horizon, the quadrant is held truly perpendicular to the horizon, and consequently in a proper position for taking the Sun's altitude. But, if the axis of vision be directed to, and turned round a point in the horizon beside the vertical circle passing through the Sun, the Sun's image, when its motion is parallel to the horizon, will be neither in the axis of vision nor the Sun's vertical, but between both; at the same time, the plane of the quadrant will not be vertical, and the altitude found by bringing the Sun's image to touch the horizon will not be the true altitude.

- II. If the quadrant be held perpendicular to the horizon, and turned about upon a vertical axis, or one nearly so, the Sun will describe an arch convex downwards in the fore-observation, and upwards in the back-observation, the motion of the Sun being the same way as the axis of vision is carried in both cases, and being to the angular motion of the quadrant, as the versè-sine of the Sun's altitude to the radius in the fore-observation, but as the versè-sine of the supplement of the Sun's altitude to 180° to the radius in the back-observation. The Sun therefore will move slower than the axis of vision in the fore-observation, and consequently will be left behind, with

with respect to the axis of vision, or seem to move backwards; and the Sun will move quicker than the axis of vision in the back-observation, or will seem to get before it. When the motion of the Sun in this arch is parallel to the horizon, the plane of the quadrant coincides with the vertical circle passing through the Sun, and consequently the quadrant is in a proper position for taking the Sun's altitude. But if the quadrant be held a little deviating from the perpendicular position to the horizon, and turned about upon an axis, either vertical or only nearly so, the arch described by the Sun apparently will cut the horizon, but will never move parallel to it, and consequently the quadrant will not be brought into a proper position for observing the Sun's altitude.

III. If the quadrant be turned on the line going to the Sun as an axis, the reflected Sun will be kept constantly in the axis of vision; and will describe an arch of a parallel circle about the real Sun, with a velocity which is to the angular motion of the quadrant, as the sine of the Sun's altitude is to the radius; and when the motion of the reflected Sun is parallel to the horizon, the quadrant is vertical.

Hence naturally arise the three methods of taking an altitude, which have been mentioned before. In the first, the axis of vision is supposed always directed to one and the same part of the horizon, namely that which is in the Sun's vertical. In the second, the observer is required to hold the quadrant truly vertical, and to turn himself upon a vertical axis;

but it is evident neither of these motions can be accurately performed. In the third method, the observer is only required to move both himself and the quadrant, so as to keep the Sun always in or near the axis of vision, which may be performed very well, because the axis of vision is a visible and certain direction for it. One exception, however, should be made to this general rule, namely, in taking the Sun's altitude when very low, by the back observation; in which case it will be best to use the second method, or else to hold the quadrant perpendicular by judgment; which will be much facilitated by using a telescope containing wires in its focus parallel to the plane of the quadrant, as described in p. 106: for, in this case, the perpendicular position of the quadrant cannot be attained so near by the method of turning the quadrant on a line going to the Sun as an axis, as it can by the other method.

It remains to treat of the errors which may arise from a defect of parallelism in the two surfaces of the index-glass, and to point out the means of obviating them in the celestial observations. It is well known, that if a pencil of parallel rays falls upon a glass whose two surfaces are inclined to one another, and some of the rays are reflected at the fore-surface, and others passing into the glass and suffering a reflection at the back-surface and two refractions at the fore-surface emerge again from the glass, these latter rays will not be parallel to those reflected at the fore-surface, as they would have been if the surfaces of the glass had been parallel, but will be inclined to the same. I find that the angle of their mutual inclination, which may be called the deviation of the rays

rays reflected from the back-surface, will be to double the inclination of the surfaces of the glass (which is here supposed to be but small), as the tangent of the angle of incidence out of air into glass, is to the tangent of the angle of refraction. Hence, in rays falling near the perpendicular, the deviation will be about three times the inclination of the surfaces; and if the angles of incidence be 50° , 60° , 70° , 80° or 85° , the deviations of the reflected rays will be about 4, 5, 7, 13 or 26 times the inclination of the surfaces, respectively. Had the deviation been the same at all incidences of the rays on the index-glass, no error would have been produced in the observation; because the course of the ray would have been equally affected in the adjustment of the instrument, as in the observation. But, from what has been just laid down, this is far from being the case, the deviation increasing according to the obliquity with which the rays fall upon the index-glass; so that in very oblique incidences of the rays, such as happen in measuring a large angle by the fore-observation or a small angle by the back-observation, the least defect in the parallelism of the planes of the two surfaces of the index-glass may produce a sensible error in the observation.

What is here said only takes place in the fullest extent, if the thickest or thinnest edge of the index-glass, or, to express the same thing in other words, the common section of the planes of the surfaces of the index-glass stands perpendicular to the plane of the quadrant; but, if the common section of the planes is inclined to the plane of the quadrant, the error arising from the defect of the parallelism of the

surfaces will be lessened in the proportion of the sine of the inclination to the radius; so that at last, when the common section becomes parallel to the plane of the quadrant, the error intirely vanishes. For this reason, Mr. Hadley very properly directed the thickest and thinnest edges of the index-glass to be placed parallel to the plane of the quadrant. But, as it may well be questioned whether this care is always taken by the instrument-maker, and it cannot be supposed that the glasses can be ground perfect parallel planes, it would certainly be an advantage acquired to the instrument, could the error arising from a want of parallelism of the planes be removed in whatever position the common section of the planes should be placed with respect to the plane of the quadrant. This will be effected for celestial observations, if the upper part of the index-glass be left unsilvered on the back, and made rough and blacked, the lower part of the glass being silvered as usual, which must be covered whenever any celestial observations are made. Then, if the telescope be sufficiently raised above the plane of the quadrant, it is evident that the observations will be made by the rays reflected from the fore-surface of the upper part of the index-glass, and consequently, if the quadrant be adjusted by making use of the same part of the index-glass, the observations will be true whether the two surfaces of the index-glass are parallel planes or not. The Sun or Moon may be thus observed by reflection from the unsilvered parts of the index-glass and horizon-glass, so that a paler darkening glass will suffice, and they will appear much distincter than from an index-glass wholly silvered with a deeper darkening

ing glaſs; for although the ſurfaces of a glaſs may be parallel, yet there always ariſes ſome little confuſion from the double reflection. Neither will the Moon appear too weak by two unſilvered reflections, even when her crescent is very ſmall, except ſhe ſhould be hazy or clouded; and then the light may be increaſed by lowering the telescope ſo as to take in part of the ſilvered reflection of the index-glaſs, which in this caſe muſt be uncovered: the ſame is alſo to be underſtood with reſpect to the Sun, ſhould his light be too much weakened by hazineſs or thin clouds. The horizon-glaſſes ſhould be adjusted, or the error of adjustment found by the Sun or Moon; the firſt will be in general the beſt object for the purpoſe; and, as the Sun or Moon ſeen directly through the unſilvered part of the horizon-glaſs will be much brighter than the image of the ſame ſeen by two unſilvered reflections, it muſt be weakened by a darkening glaſs placed beyond the horizon-glaſs, the reflected image being farther weakened, if neceſſary, by a paler darkening glaſs placed in the uſual manner between the index-glaſs and the horizon-glaſs.

If a quadrant was deſigned principally for taking the diſtance of the Moon from the Sun and fixed ſtars, and was not wanted for obſerving terreſtrial angles, it would be the beſt way to have none of the glaſſes ſilvered, but to leave the horizon glaſſes intirely tranſparent, and to put a red glaſs for an index-glaſs of the ſame matter with the darkening glaſſes, which would reflect light from the fore-ſurface only.

The Sun's altitude might alſo be obſerved with this inſtrument, either by the fore or back-obſervation;
and

and the altitude of the Moon might be taken with it in the night. But the altitudes of stars could not be observed with it, nor the Moon's altitude in the day time, which would however be no great inconvenience, as these observations might be well enough supplied by common quadrants.

The following rules for the size of the glasses and the silvering them, and the height of the telescope may be of use. The index glass and two horizon-glasses should be all of equal height, and even with one another in height both at top and bottom. The telescope should be moveable parallel to itself nearer to or farther from the plane of the quadrant, and the range of its motion should be such that its axis when at the lowest station should point about $\frac{1}{10}$ th of an inch lower than the top of the silvering of the horizon-glasses, and when at the highest station should point to the height of the middle of the unsilvered part of the index-glass. The height of the glasses, and the quantity of parts silvered and parts unsilvered, should vary according to the aperture of the object-glass, as in the following table; where the first column of figures shews the dimensions in parts of an inch answering to an aperture of the object-glass of $\frac{1}{10}$ ths of an inch in diameter; the second column what answer to an aperture of the object-glass of $\frac{1}{8}$ ths of an inch in diameter; and the third, what are suitable to an aperture of the object-glass of $\frac{3}{16}$ ths of an inch in diameter.

	Parts of an Inch		
Diameter of aperture of object-glass	.30	0,40	0,50
Height of glasses	.90	1,13	1,37
Height of silvered part of index-glass	.50	0,63	0,77
Height of unsilvered part of ditto	.40	0,50	0,60
Height of silvered part of horizon-glasses	.25	0,33	0,42
Height of unsilvered part of ditto	.65	0,80	0,95

If the telescope has a common object-glass, the first aperture of $\frac{1}{10}$ ths of an inch will be most convenient; but if it has an achromatic object-glass, one of the other apertures of $\frac{4}{10}$ ths or $\frac{5}{10}$ ths of an inch, will be most proper. The field of view of the telescope should be 5 or 6 degrees, and the objects should be rendered as distinct as possible throughout the whole field; by applying two eye-glasses to the telescope. The breadth of the glasses should be determined as usual, according to the obliquity with which the rays fall on them and the aperture of the object-glass.

I shall conclude this paper with some easy rules for finding the apparent angular distance between any two near land objects by the Hadley's quadrant.

To find the angular distance between two near objects by the fore-observation. Adjust the fore-horizon-glass by the object intended to be taken as the direct object; and the angle measured by the fore-observation on the arch of the quadrant between this object and any other object seen by reflection will be the true angle between them as seen from the centre of the index-glass. But, if the quadrant be already

already well adjusted by a distant object, and you do not chuse to alter it by adjusting it by a near one, move the index, and bring the image of the near direct object to coincide with the same seen directly, and the number of minutes by which (o) of the index stands to the right hand of (o) of the quadrant upon the arch of the excess is the correction, which added to the angle measured by the arch of the quadrant between this direct object and any other object seen by reflection will give the true angular distance between them reduced to the centre of the index-glass.

To find the angular distance between two near objects by the back-observation.

It is supposed that the-horizon-glass is truly adjusted; if it is not, let it be so. Observe the distance of the objects by the back-observation, and take the supplement of the degrees and minutes standing upon the arch to 180 degrees, which call the instrumental angular distance of the objects; this is to be corrected as follows. Keep the centre of the quadrant or index-glass in the same place as it had in the foregoing observation, and observe the distance between the near object, which has been just taken as the direct object, and some distant object, twice; by making both objects to be the direct and reflected ones alternately, holding the divided arch upwards in one case and downwards in the other, still preserving the place of the centre of the quadrant. The difference of these two observations will be the correction, which added to the instrumental angular distance,

distance, found as above in the first observation between the first object and any other object seen by reflection, will give the true angular distance between them reduced to the centre of the index-glass.

But if you should happen to be in a place where you cannot command a convenient distant object, the following method may be used.

The back-horizon-glass being adjusted, find the instrumental angular distance between the objects; this is to be corrected by means of the following operations. Set up a mark at any convenient distance opposite or nearly so to the object which has been taken as the direct object; and looking at the direct object move the index of the quadrant, and bring the image of the mark to coincide with the direct object, and read off the degrees and minutes standing on the arch of the quadrant, which subtract from 180 degrees, if (o) of the index falls upon the quadrantal arch; but add to 180 degrees, if it falls upon the arch of excess; and you will have the instrumental angular distance of the object and mark. Invert the plane of the quadrant, taking care at the same time not to change the place of its centre, and looking at the same direct object as before, move the index of the quadrant, and bring the image of the mark to coincide again with the direct object, and read off the degrees and minutes standing on the arch, and thence also find the instrumental angular distance of the object and mark. Take the sum of this and the former instrumental angular distance; half of its difference from 360° will be the correction, which added to the instrumental angular distance first found between the same direct object and the other object seen

by reflection will give the true angular distance between them reduced to the centre of the index-glass.

It is to be observed, that if the mark be set up at the same distance from the quadrant as the direct object is, there will be no occasion to invert the plane of the quadrant, but the observer need only make the image of the mark coincide with the direct object, then turn himself half round, and now taking the mark for the direct object cause the image of the former direct object to coincide with the mark, the divided arch of the quadrant being kept upwards, and the place of the centre of the quadrant remaining also the same in both cases: half the difference of the sum of the two instrumental angles from 360 degrees will be the correction of the adjustment as before.

Should only one of the objects be near, and the other remote (that is to say, half a mile distant or more) let the distant object be taken for the direct one, and the near object for the reflected one; and the true distance of the objects as seen from the centre of the index-glass will be obtained without requiring any correction, whether it be the back or fore observation that is made use of; only observing, as usual, to take the supplement of what is shewn upon the arch to 180 degrees in the back-observation.

XV. *Account of the Irruption of Solway Moss in December 16, 1772; in a Letter from Mr. John Walker, to the Earl of Bute, and communicated by his Lordship to the Royal Society*.*

My Lord,

Read Feb. 13, 1772. **W**HEN I was fitting yesterday writing to your Lordship, I received the honour of yours. I shall therefore defer the account I intended of my expedition last season to the north, and give the best description I can, of the extraordinary irruption of Solway-moss, which I went to visit, about a week after it happened.

It is not surprizing, that it has every where attracted the attention of the public; for though the cause of it is obvious, yet so far as I recollect, the alteration it has produced on the face of the earth, is greater than any we have known in Britain, from natural causes, since the destruction of Earl Goodwin's estate.

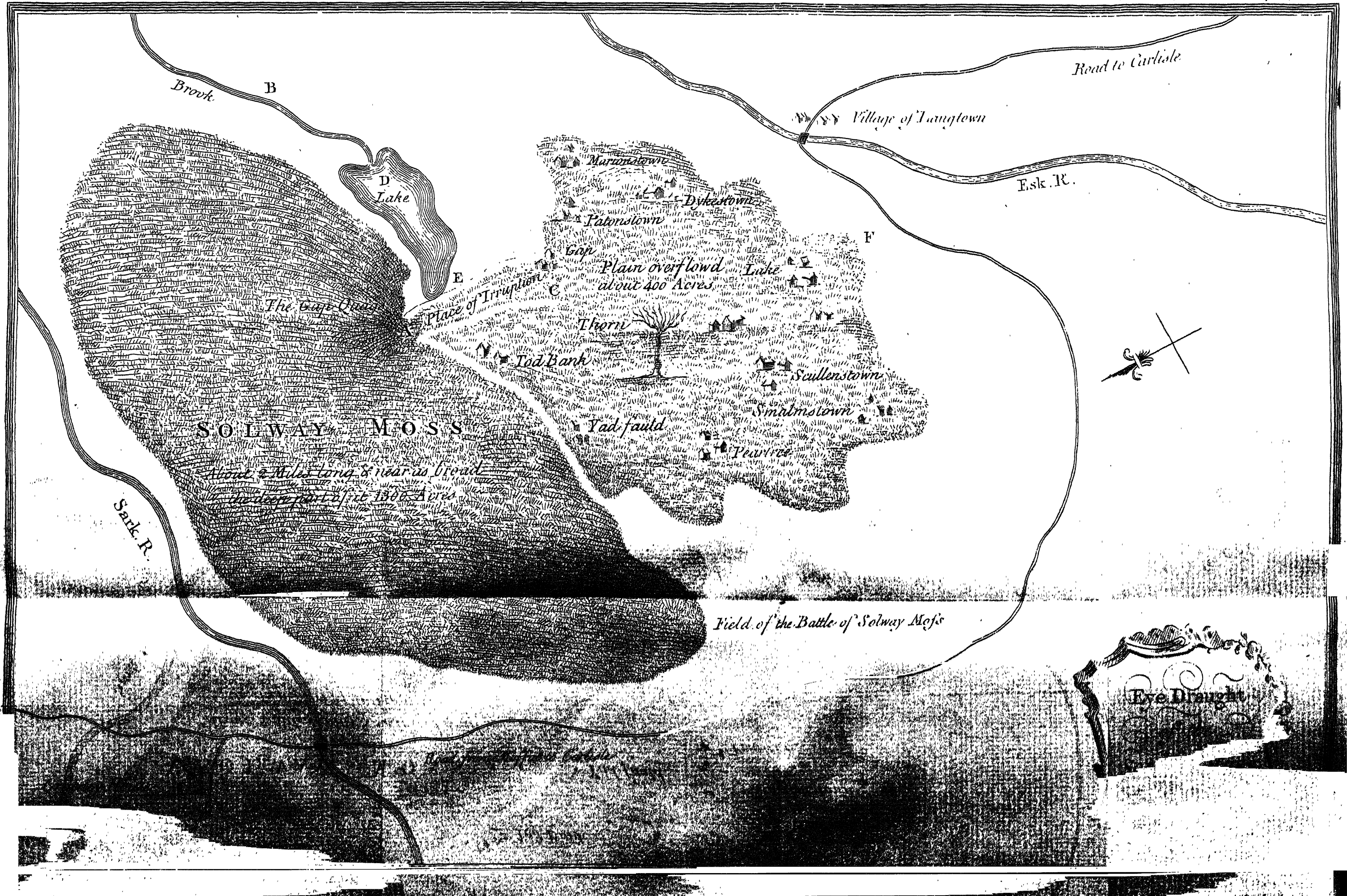
* The Society has received, from other hands, several accounts of this curious and singular phænomenon; but this, as one of the latest, being likewise the completest, was thought the most proper to be laid before the public; especially as, on comparison, few particulars of any importance mentioned in the other accounts were found wanting in this. These few, however, have been collected, and subjoined in the form of notes. M. M.

It happened on the 16th of December, when there fell such a deluge of rain, over all the North of England, as has not been known, for at least two hundred years. There was a very great flood at Moffat, but I think, I have seen one or two greater, and certainly it was not so extraordinary here, as further South.

The Solway flow contains 1300 acres of very deep and tender moss, which, before this accident, were impassable, even in summer, to a foot passenger. It was mostly of the quag kind, which is a sort of moss covered at top with a turf of heath and coarse aquatic grasses; but so soft and watery below, that, if a pole is once thrust through the turf, it can easily be pushed, though perhaps 15 or 20 feet long, to the bottom. If a person ventures on one of these quags, it bends in waves under his feet; and if the surface breaks, he is in danger of sinking to the bottom*. The surface of the flow was, at different places, between 50 and 80 feet higher than the fine fertile plain, that lay between it and the river Esk. See Tab. VI. About the middle of the flow, at the place marked A, were the deepest quags, and there the moss was elevated higher above the plain, than in any part of the neighbourhood. From this, to the farm called the Gap, upon the plain at C, there was a broad gully,

* The surface was always so much of a quagmire, that, in most places, it was hardly safe for any thing heavier than a sportsman to venture upon it, even in the driest summers. A great number of Scotchmen, in the army commanded by Oliver Sinclair in the time of Henry VIII. lost their lives in it; and it is said that some people digging peats upon it, met with the skeleton of a trooper and his horse in complete armour, not many years ago.

though



though not very deep, through which the brook marked B used to run. The moss being quite overcharged with the flood, burst at these quags, about 11 o'clock at night, and finding a descent at hand, poured its contents through the gully into the plain.

It surprized the inhabitants of 12 towns in their beds*. Nobody was lost, but many of the people saved their lives with great difficulty. Next morning, thirty-five families were found dispossessed, with the loss of most of their corn and some cattle†. Some of the houses were near totally covered, and others of them I saw standing in the moss, up to the thatch, the side walls being about 8 feet high.

In the morning, above 200 acres were entirely overwhelmed; and this body of moss and water, which was of such a consistency, as to move freely, continued to spread itself on all hands, for several days. It was come to a stop, when I saw it, and had covered 303 acres, as I was informed by a gentleman, who had looked over the plans of the grounds, with Mr. Graham the proprietor: but every fall of rain sets it again in motion, and it has now overspread above 400 acres. At F, it had run within a musket shot of the post road leading from

* Those who were nearest the place of bursting were alarmed with the unusual noise it made; others not till it had entered their houses, or even, as was the case with some, not till they found it in their beds.

† The case of a cow seems singular enough to deserve a particular mention. She was the only one of eight in the same cow-house, that was saved, after having stood sixty hours up to the neck in mud and water. When she was got out, she did not refuse to eat, but water she would not taste, nor could even look at, without shewing manifest signs of horror. She is now reconciled to it, and likely to recover.

Moffat

Moffat to Carlisle, when I saw it, but it is now flowed over the road, and reached the Esk. This river, which was one of the clearest in the world, is now rendered black as ink, by the mixture of the moss, and no salmon has since entered into it. A farmer also told me, that, upon removing the moss, to get at a well which it had covered, they found all the earth-worms lying dead upon the surface of the ground. The land, that is covered, was all inclosed with hedges, bore excellent crops of wheat and turnips, and rented from between 11 and 14 shillings, besides the taxes and tithes, which amounted to 4 shillings per acre.

I endeavoured to guess at the depth of the moss upon the plain, by a large thorn, which stands in the middle of it, and which is buried to above the division of the branches. The farmers told me, that it stood upon a rising, more than 6 feet above the general level of the plain; and that it was upwards of 9 feet high, of clear stem. By this account, great part of the plain must be covered 15 feet deep with the moss: and near the farm called Gap, there were some considerable hollows, where they think the moss, at present, lies full 30 feet deep. The tallest hedges on the land are all covered over the top. The houses are not so much buried, because they stood mostly on the higher parts of the fields; and, towards the extremities of the moss, I observed it, in many places, not above 3 or 4 feet deep, owing likewise to the rising of the ground.

The gut at A, through which the whole of the moss flowed that covered the plain, is only about

50 yards wide, and the gully from A to C is near a quarter of a measured mile long.

The brook B, being stopped up by the moss at E, has now formed a lake at D.

About 400 acres of the flow, next the place of its evacuation, appear to have sunk from 5 to 25 feet: and this subsidence has occasioned great fissures upon those parts of the moss which refused to sink. These fissures are from 4 to 8 feet wide, and as much in depth. The surface of the flow, consisting of heath and coarse grass, was torn away in large pieces, which still lie upon the surface of the new moss, some of them from 20 to 50 feet long. But the greater part of the surface of the flow remained, and only subsided; the moss, rendered thin by the flood, running away from under it.

Looking over the Solway moss, at the village of Longtown, where there is a bridge on the Esk, they formerly saw only the tops of the trees at Gratney, a house of the Marquis of Annandale's, 4 miles distant; but now they see them almost to the ground. And looking over it, in another direction, they now see two farm-towns of Sir William Maxwell's, which were not before visible. So that the ridge of the flow or moss seems to have subsided about 25 feet.

I ever am, with the highest respect,

My Lord,

Your Lordship's most obliged

and devoted servant,

Mossat,

Jan. 30, 1772.

John Walker.

XVI. A

XVI. *A Letter from John Zephaniah Holwel, Esq; F. R. S. to John Campbel, Esq; F. R. S. giving an Account of a new Species of Oak.*

Exeter, Feb. 24, 1772.

S I R,

Read April 1, 1772. **I**N my curious rambles through the environs of this city, I have been tempted to visit the nursery of Mr. William Lucombe, of St. Thomas, on the report of a very extraordinary and new species of oak, first discovered and propagated by that ingenious gardener; and as this plant appears to me capable of proving an inestimable acquisition to this kingdom, I cannot resist the desire I feel of communicating to you some particulars relative to its history and character, taken partly from Mr. Lucombe's account of it, and my own observations. This, I know, must be most acceptable to you, who are so laboriously and laudably employed in elucidating the various improvements and advantages your country is capable of.

About seven years past, Mr. Lucombe sowed a parcel of acorns, saved from a tree of his own growth, of the iron or wainscot species; when they came up, he observed one amongst them that kept
his

his leaves throughout the winter ; struck with the phænomenon, he cherished, and paid particular attention to it, and propagated, by grafting, some thousands from it, which I had the pleasure of seeing, eight days ago, in high flourishing beauty and verdure, notwithstanding the severity of the winter. Its growth is straight, and handsome as a fir, its leaves ever-green, and the wood is thought, by the best judges, in hardness and strength to exceed all other oak. He makes but one shoot in the year, viz. in May, and continues growing without interruption ; whereas other oaks shoot twice, namely, in May and August ; but the peculiar and estimable part of its character is, the amazing quickness of its growth, which I imagine may be attributed (in some degree at least) to its making but one shoot in the year ; for I believe all trees that shoot twice, are, for some time, at a stand before they make the second. I had the curiosity to take the dimensions of the parent tree (seven years old), and some of the grafts ; the first measured 21 feet high, and full 20 inches in the girt ; a graft of four years old 16 feet high, and full 14 inches in the girt ; the first he grafted is six years old, and has out-shot his parent 2 feet in height. The parent tree seems to promise his acorns soon, as he blossoms, and forms his foot-stalk strong, and the cup upon the foot-stalk with the appearance of the acorn, which, with a little more age, will swell to perfection. This oak is distinguished, in this county, by the title of The Lucombe Oak ; his shoots, in general, are from 4 to 5 feet every year, so that he will, in the space of thirty or forty years, out-grow in altitude and girt the common oak at a

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hundred. In two or three days I will forward to you, in a parcel, a branch, which I cut off from the original tree, and another from the graft of four years old, also a dead branch of the iron or wainscot oak, just to shew that, from the similarity of the leaves, it is a descendant from that species, although differing from it in every other particular. I send you also, by the Exeter stage, a specimen of the wood. I have a walking-pole full 5 feet long, a side shoot from one of the grafts, only one year and half old. Several gentlemen round this neighbourhood, and in the adjoining counties of Cornwall and Somerset, have planted them, and they are found to flourish in all soils.

I am, Dear Sir,

Your faithful friend, and

most obedient humble servant,

J. Z. Holwell.

XVII. An Account of the Death of a Person destroyed by Lightning in the Chapel in Tottenham-Court-Road, and its Effects on the Building; as observed by Mr. William Henly, Mr. Edward Nairne, and Mr. William Jones. The Account written by Mr. Henly.

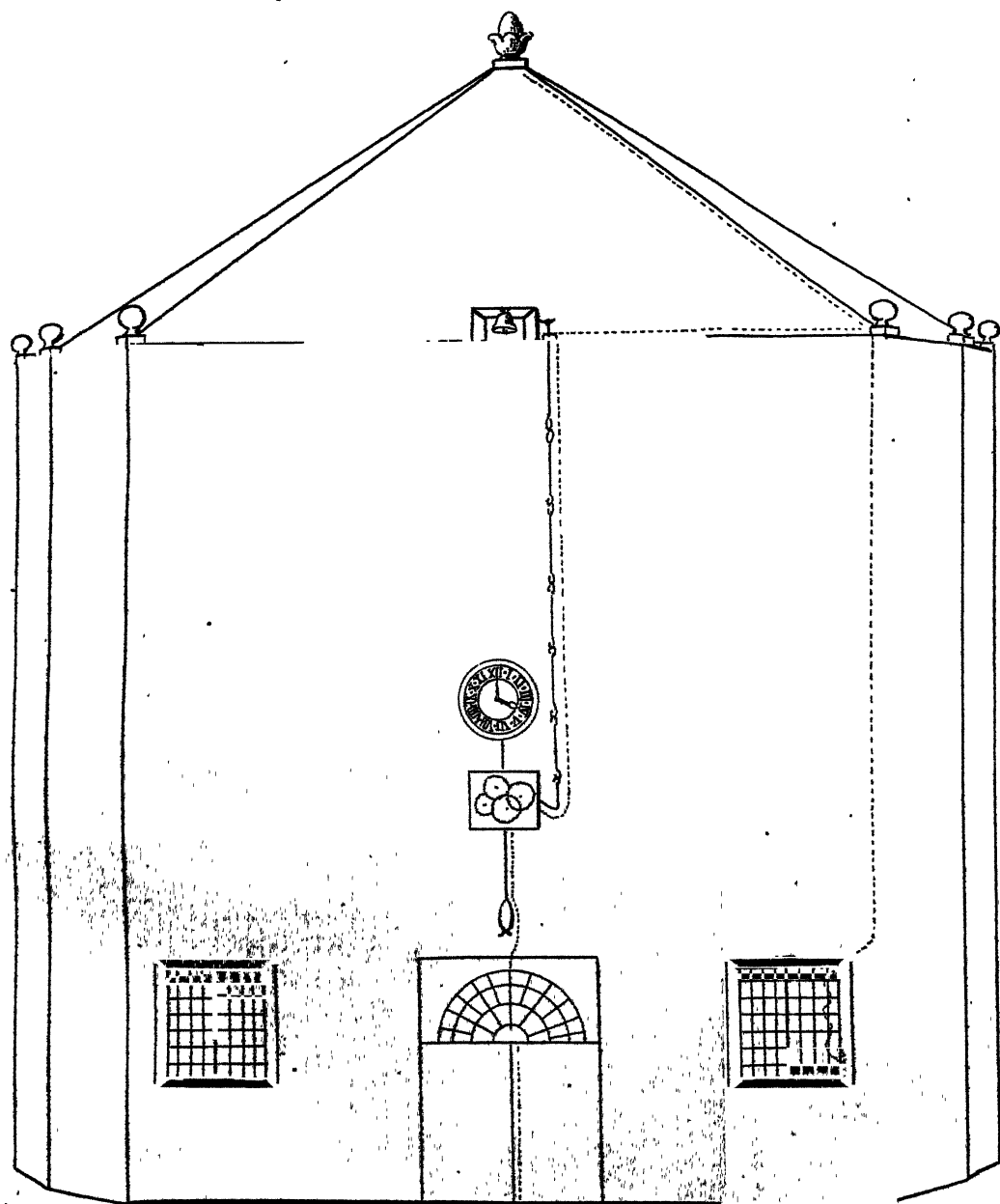
March 24, 1772.

Read April 9, 1772. **O**N Sunday last, exactly at 4 o'clock, P. M. part of a building erected by the late Rev. Mr. Whitfield, in Tottenham-court-road, commonly called the Chapel or Tabernacle, was struck by a flash of lightning. This part was an addition afterwards made to the original structure, but was greatly inferior to it in height. On its summit stood an ornament representing a pine-apple carved in wood, which consisted of two pieces; the uppermost being connected with the lower by means of several iron spikes. It was supported by a strong plinth of wood covered with lead lapped over the edges and corners of its top, and there secured by large iron nails. This lead work was connected with that which covered the hips, and made a regular communication of metal, to the bottom of the slating, where it united with a leaden gutter which extended

quite round the building. In this gutter was erected a small lanthorn, in which hung the bell of the clock. A little pipe of lead was soldered to, and extended perpendicularly a few inches above, the surface of the gutter; through this pipe went a small iron wire consisting of many long links, connected with the tail of the hammer; passing thence within a few inches of the striking rod of the clock, to which it was tied by a strong hempen string 6 inches or more in length. The lightning first struck the pine-apple, the upper part of which it shivered into very small fragments, and threw them in all directions from the place, and melted off the end of one of the spikes. It left a smoky track upon the under-part of it, and then struck the edge of the lead upon the plinth, which it melted in two places, quite through the substance of it. A little below these I found a third spot; this was melted in a very regular and curious concave about an $\frac{1}{4}$ th of an inch diameter at the surface, with a small perforation at the bottom, through which I think might have been introduced one of the finest sort of sewing needles. The whole figure somewhat resembled a small funnel *. It passed thence by a regular communication of metal, till it reached the wire of the clock hammer before spoken of, melting it about half through its diameter, which, in this place, was less than the twelfth part of an inch. The edge of the lead pipe from which it leaped to the wire was also much melted. The wire was melted at every juncture of the links; the packthread at the bottom was but little injured, but the electric matter leaped through

* Quere, is not this a token of the stroke's being from the clouds downwards?

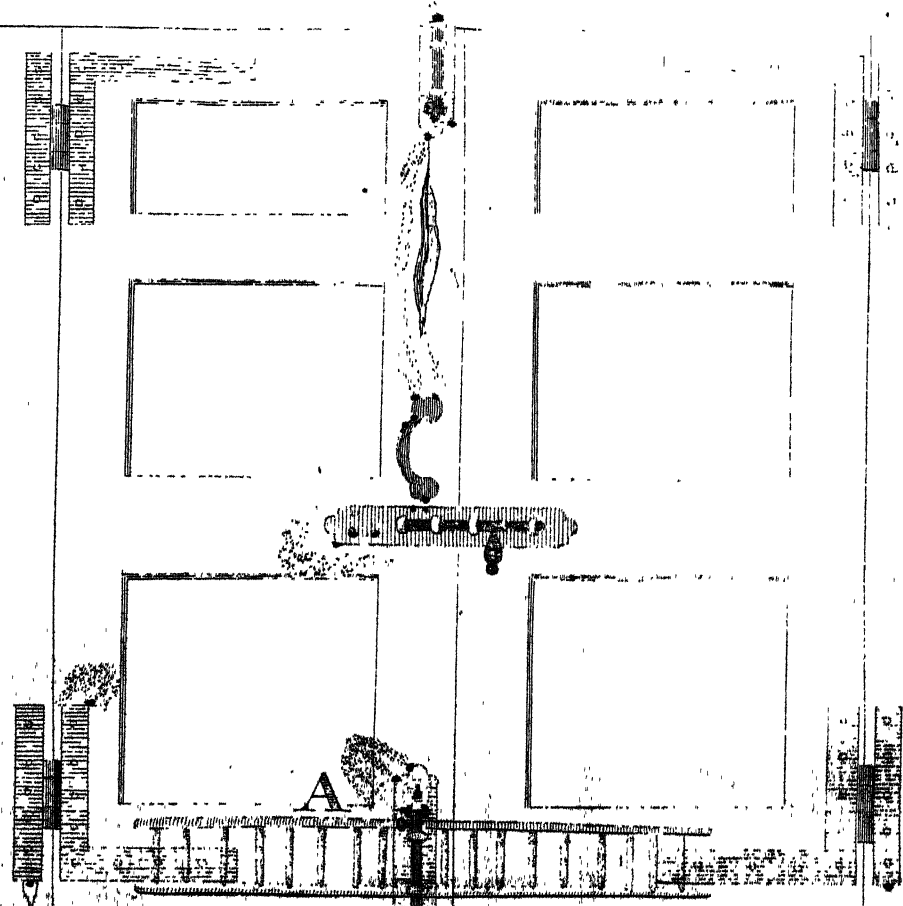
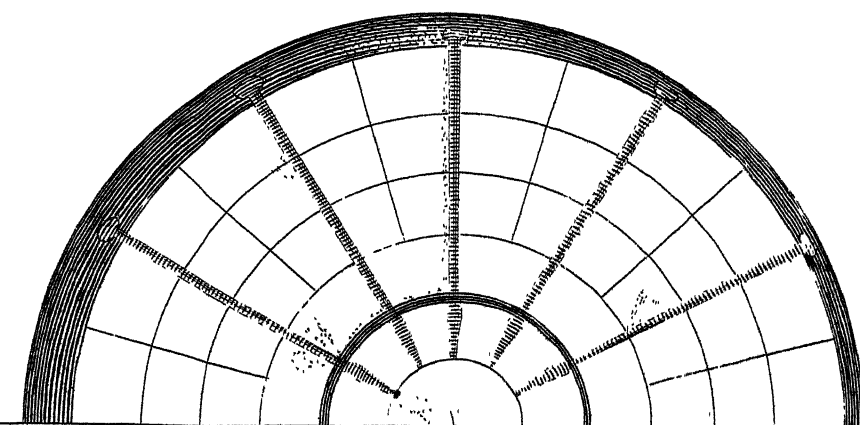
a few



c. An outside view of that part of the Building which was affected by the Lightning.

V.B. The dotted Line represents the course the Electricity took in its passage to the ground.

The dots on the cinn work & the five strokes represent the marks left at those places by the Lightning. The person destroyed sat on the Sadder at A.



a few inches of air to the striking rod of the clock, in which, near the end, it melted a large spot, whence it was conducted by the work of the clock to the upper part of the pendulum, in the axis of which it melted another large spot, and descended by the rod passing over the ball, which it melted in a most remarkable manner in six or seven places (perhaps upon the ball it might accumulate, and, for want of a proper conveyance, break out in different parts of it) and quitted it at the bottom of the nut, which is melted in three places, and will accompany this paper. Here the electricity leaped through eight inches of air, or passed in conductors of the worst kind, dry brick and wood (with a considerable cavity between them), till it reached the frame of a window, over the doors, where it broke the ceiling, and burnt the wood to a coal. Here it met with the point of a nail, driven upward into the window frame as a security to the center bar. The point of this nail is melted off, I suppose, full half an inch; it was also melted in two large spots on the opposite sides near the head. My friend Mr. Jones drew it from the bar, &c. This gentleman was also so obliging as to take down a sketch of the window, and an outline of the parts affected of the building. [See Tab. VII.] The nail is now in the custody of Mr. Nairne. The lightning passed down the aforementioned bar, and by a bent iron (in contact with both), into another bar, whose point (which was greatly melted) came much nearer the upper bolt of the door. The lead-work, from the point of the bar was melted, and a board nearly in contact with the staple of the bolt much blacked by the passing of the electricity.

Here

Here it struck the upper edge of the staple, which projected a little above the top of the bolt, melted it in a most extraordinary manner; the spot, and indeed several others, having run into a kind of spiral form, which is raised considerably, as may be plainly distinguished by a very shallow magnifier, and often, as in this, by the naked eye. This effect was first observed by Mr. Nairne. When it quitted this bolt, it struck upon a semicircular handle of iron (first tearing out a large piece from the door), the upper part of which hath three melted spots, besides a single one at the upper edge of it. But, in quitting it, the electricity melted only one spot at the lower edge *, which I think, as Mr. Bell (a gentleman who was with us) observed, was a criterion whereby to judge of the direction of the fluid. To the left of this door, at the distance of eleven feet four inches, came down a leaden pipe, which terminated at the ceiling, and there just entered a pitched trunk of fir (which indeed was the case with every leaden pipe about the building). Here the lightning exploded, rending the trunk, and doing other slight damage in and about a window, to which it was attracted by an interrupted and irregular communication of metal. I would beg leave to remark, that, had this pipe of lead been continued to the bottom of the building, and thence conveyed into the earth, in the manner directed by Dr. Franklin, I can have no doubt but the whole contents of the explosion would have passed this way, have been

* Quere, is not this effect somewhat analogous to Mr. Lullin's electrical experiment with a card?

conducted with perfect safety to the building, &c. and that no other part of it would have been at all affected. As the effects of this stroke so exactly correspond with those many times before observed by Dr. Franklin, I think we shall scarce ever meet with a greater proof of the utility of his metallick conductors; and cannot help expressing a sincere wish, that builders, and persons engaged in the erection of public edifices, &c. might be prevailed with, to make a regular communication of metal, from the top of such buildings to a considerable depth into the earth, and of such a diameter and kind, as may be sufficient to secure both the buildings, and the lives of those, who may happen to be in them. The poor man destroyed by this accident, was sitting at the time on a short ladder, which lay horizontally on the pavement, with his back against the door. The lightning flew from the middle bolt, and struck him on and under his left ear, entered his neck, making a wound half an inch long, raised in a bur and burnt, passed down his back, which it turned black as ink, down his left arm, melting the stud in his shirt sleeve; the stone in which, as well as the silver, seems to be a little affected. Hence it flew into his body, which it burnt in a hard spot, resembling scorched leather, passing through it into his right leg, and breaking out a little above the ankle; making a large wound, and another bur, burnt as before, with two others smaller a little below it, and some still smaller in his feet. His cloaths and hair were much burnt, but his stock, shoe, and knee-buckles, the metal buttons on his coat and waistcoat, a shilling, which he had in the left

left pocket of his breeches, and the metal clasps of a Common Prayer-Book, in his coat-pocket, were all uninjured *. His death was truly instantaneous; he hath left a widow and two children in distressed circumstances, who were entirely dependent on his labour. His name was Goodson, aged thirty-four, by trade a taylor, at N^o 3. Craven-Buildings, Drury-Lane.

P. S. The studs above-mentioned will be sent together with this paper (as a curiosity) for the inspection of the members of the Royal Society.

* The corps, after lying two or three days on a table, seemed not more disposed to putrefaction, than bodies at that time generally are, which die a natural death.

XVIII. *A Letter from Thomas Ronayne, Esq; to Benjamin Franklin, LL. D. F. R. S. inclosing an Account of some Observations on Atmospherical Electricity; in regard of Fogs, Mists, &c. with some Remarks; communicated by Mr. William Henley.*

S I R,

Read April 30, 1772. **I**N conformity to the desire of some friends, I have drawn up the following observations on atmospherical electricity, which I beg leave to lay before you; and shall think the trouble I have had, in prosecuting the necessary experiments, sufficiently compensated, if it shall appear to you that they contain any thing new or curious; in which case, you are at liberty to dispose of them in whatever manner you shall think proper.

I am, S I R,

With very respectful consideration,

Your most obedient servant,

Cecil-Street,
Feb. 15, 1772.

Tho^s Ronayne.

* SOME years ago I discovered, by Mr. Canton's electrometer, described in the *Philos. Transactions*, Vol. XLVIII. p. 783. that the air of Ireland is, during the winter season, in almost a constant state of positive electricity; which, however, is so weak, that, in order to observe it satisfactorily, I have always found it necessary to have the cork-balls suspended from threads of a middling fineness, six or seven inches in length, quite streight, and to avoid, as much as possible, any interruption from the wind.

I have likewise had frequent recourse to the following contrivance, by which I was enabled, within doors, to pursue my inquiries with greater accuracy and advantage: having procured a slender tapering piece of wood, about five feet long, to the smaller end of which an electrometer was affixed, by means of a small hook; I placed it out from an open garret window, and fastened the other end with a small hasp to one of the jambs; I had also at hand another piece of wood, in the ends of which, a small glass tube and a stick of sealing-wax had been inserted. Either of these was occasionally excited, and applied near the cork-balls, in order to determine more precisely the kind of electricity with which they might happen to be affected; and I was always careful in

* I commenced my experiments on the air in the year 1761, and those on the clouds in the year 1762. I continued them unremittingly, as opportunity offered, till the year 1770, when my occasions brought me to England. I have repeated my experiments, with regard to the common air, in different places here, and find it the same as that in Ireland.

making

making my experiments on that side of the house where the wind had least power.

I have found the air, in winter, at a proper distance from buildings, trees, masts of ships, &c. very sensibly electrified, during a frosty or foggy state of the weather; and in mists too, but in a less degree: I have also discovered small signs of it in calm and cloudy weather.

The air, in summer, never shewed any sign of electricity, except when a fog happened in the cool of the evening, or at night; in which case, I always discovered manifest marks of electricity, sensibly weaker than those observed in winter fogs, but precisely of the same kind, that is, *positive*.

I have often examined the state of the air, at the time of an Aurora borealis, and could not discover any indication of electricity, except when a fog had appeared at the same time; in which case, the electricity has been, in every respect, the same as that of a fog at any other time. Once, indeed, during an Aurora borealis on a remarkable serene night, I discovered some signs of a very weak *positive* electricity.

As the electricity of the air is generally *positive* (I never knew an exception but one, which presented itself during a fog on a winter day, that proved uncommonly warm), is it not reasonable to believe, that cold electrifies the atmosphere *positively*? and, if so, may not one be led to imagine, that heat electrifies it *negatively*? But this I only offer as a conjecture, not being able to advance any thing decisive on the subject, and knowing that one sort of electricity may

often be productive of the other, as is plain from Dr. Franklin's experiments.

If cold electrifies the air *positively* in this climate (which seems extremely probable), may it not electrify it *negatively* at and about the place of our antipodes? Does not a consideration of the effects discovered in the Tourmalin favour this surmise?

The electricity of the air, in frosty, foggy or misty weather, is not strong enough to yield any spark, even by insulating a sharp pointed wire in it, which, however, attracts very light bodies at a small distance; whilst, on the contrary, that of the clouds generally affords considerably strong sparks.

When a fog becomes very thick, the cork-balls approach; but when it returns to its former state, they open again at their first distance; and I have observed that, when it rained in foggy weather, the balls closed, and opened again on the fog's appearance anew, after the rain had ceased: there is, however, a certain degree of density necessary in a fog, in order that the balls might exert their greatest divergency.

Moist, if not all, fogs partake of a smell much like that of an excited glass tube, and, indeed, so does the common air very frequently.

As fogs sometimes appear in a very moist state of the air, I was for some time at a loss to account on what principle they could retain their electricity; but having at length remarked, that electrified bodies, insulated with sealing-wax, preserved their electricity for a time in very damp air, I concluded that moisture is but a very slow conductor.

Having,

Having, on the contrary, observed that bodies, insulated with dried silk, had lost their electricity in a very short time, I attempted to render it a non-conductor, by having varnished it over with oil of turpentine, balsam of sulphur, and such like, but did not succeed; for silks so treated soon became a conductor, and increased considerably in weight, if the air happened not to be very * dry; so much indeed, that I think ordinary silk, from its power of absorbing moisture from the air, may well serve as an occasional hygrometer, either by being put into a balance, or by having an electrified body insulated with it.

When the density of fogs, floating near the earth, increases considerably, the balls always approach; but when they are situated high in air, the reverse generally happens. I had an opportunity of remarking a struggle between breezes from the north-west and south-east at the same time, in which the one seemed sometimes to prevail, and afterwards the other. This contention was succeeded by a smoaky haziness, which, like a fog, occasioned the balls to open: as the haziness † thickened, they opened

* Even glass attracts moisture to its surface, which makes it a conductor of electricity, and, consequently, not so convenient as sealing-wax.

† An electrical body, when contracted in its dimension, will have its electricity increased, as appears by Dr. Franklin's curious experiment with the chain and silver can. I also have discovered, from repeated trials, that a piece of flannel, silk, &c. excited, and suddenly twisted, not only struck at a greater distance than before, but sometimes emitted pencils of fire into the air. May we not hence infer why the electricity of vapour, &c. (when not in contact with the earth) increases by condensation?

wider,

wider, and still wider when it dissolved into rain; but their repelling power became greatest in proportion as the drops increased.

The electrometer placed out from a garret window (p. 138.), has been frequently useful to me, in determining the nature of an approaching cloud, whose electricity, although generally strong, was for the most part uncertain, having been sometimes *positive*, and at other times *negative*. But, as the wind or rain were frequent impediments to the accuracy of my experiments, the following methods of making observations, with success, under shelter, occurred to me.

I have sometimes stood, in an upper room, on a cake of wax, holding in my right hand, out at the window, a long slender piece of wood, round which a wire projecting a few inches had been twisted, and in my left hand an electrometer: an assistant had excited glass or wax in readiness.

At other times, I have made use of a tapering tube of tin, twenty feet long, ending in a point; the greatest part of it stood out high in the air, and the thick end, from which an electrometer hung, was supported inside the window, sometimes with silk cords, and at other times with strong sticks of sealing-wax, sustained at either end by hooks of iron-wire.

By either of these means I have often discovered, that what seemed to me a single cloud, produced, in its passing over, several successive changes, from *positive* to *negative*, and from *negative* to *positive* electricity, the balls coming together each time, and remaining in contact a few seconds, before they repelled each other again.

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The permanence of either kind of electricity in the clouds, or the length of time in which neither can be discovered, is uncertain; sometimes the same electricity has returned, and at other times has been succeeded by the contrary; whilst either generally came on, and went off gradually. But changes were often made, very suddenly, by a flash of lightning, especially if the thunder-storm happened to be in the zenith. A branch of it, over-head, has frequently occasioned stronger electricity than I could discover, when the greatest part of the sky had been overcast; which, perhaps, might be accounted for, from this consideration, that one kind of electricity, acting alone, must exert more powerful effects than when counteracted by the other.

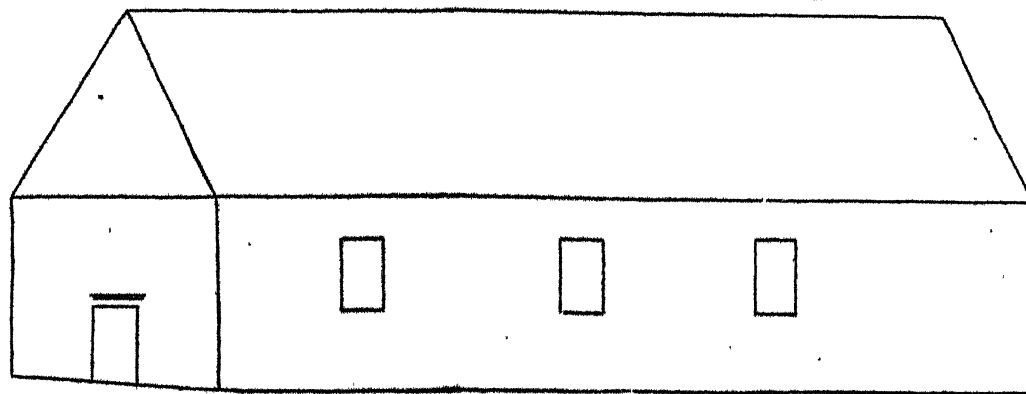
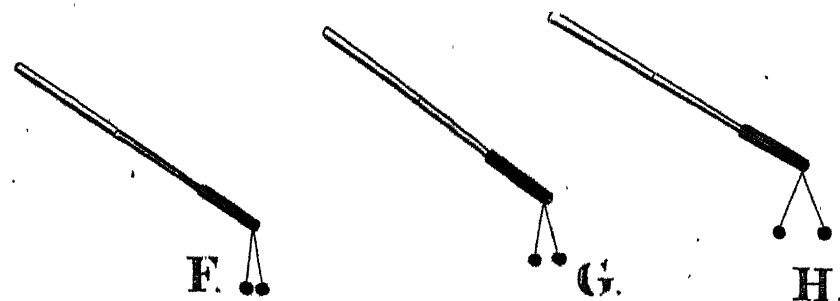
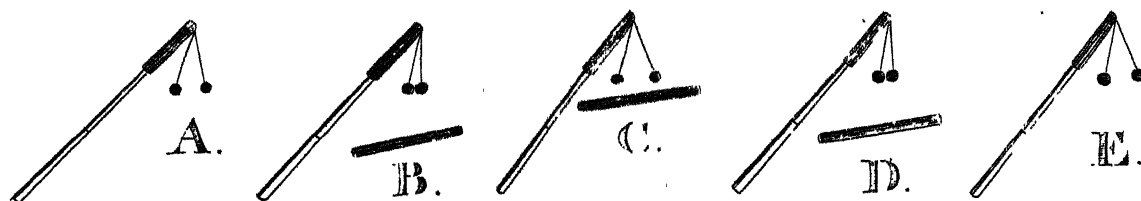
I once observed in a thunder-storm, during which I saw no lightning, that the balls, which hung from the tin tube, repelled and attracted each other, very rapidly, for the space of ten or twelve seconds; at the same time, Mr. Canton's electrometer, which I held at such a distance from the tube, as to have its balls opened to the distance of an inch, continued quiet in that state, and were not affected convulsively like the others. Hence I imagined, that the same kind of electricity went off, and came on, without being changed in *contrarium*; for when that circumstance happened, they were very evidently affected in the same manner. And here I must observe, that I have found it more easy to discover the kind of electricity present in the tube, by approaching excited wax to the balls of an electrometer, which I held at a proper distance from the tube, than by applying it near the balls which hung from the tube;

for

for they, in the general, diverged so much, that I found it very difficult to have in readiness a small tube of glass, or wax sufficiently excited to affect them.

It has sometimes happened that the balls of the tin tube, &c. perfectly at rest, have, in consequence of a flash of lightning, suddenly repelled each other, and immediately after closed. As this circumstance has frequently happened, when the air was in a damp state, I have sometimes imagined that the equilibrium between the earth and lower clouds had been quickly restored, on receiving the electricity of the higher ones; and, at other times, have supposed that it might be owing to the lateral effect of the explosion.

If two or more persons, at a sufficient distance, from each other, would correspond, by signals, viz. a red flag for *positive*, and a blue one for *negative* electricity, we should probably obtain, in due time, more satisfactory certainty with regard to the electricity of the clouds, thunder, &c. than has hitherto been given, or is, perhaps, possible for any one man to acquire, without the aid of wires or chains, produced from different apparatuses, placed at different distances from each other.



Mr. Ronayne having received the following Letter from Mr. Henly, which corroborates and confirms the observations mentioned in his paper, it was thought proper to print them together in this volume.

October 16, 1771, $\frac{1}{4}$ past 5, P. M.

I Find a fog (not very thick), soon after its appearance, strongly electrical. The balls open $\frac{1}{2}$ or $\frac{3}{4}$ inch A, [See Tab. VIII.], and close at the approach of excited wax, when brought within 10 inches of them B: if the wax is brought within 3 or 4 inches, they diverge again, in consequence thereof C: as the wax is withdrawn, they converge again, D, till it gets beyond the distance of its influence, when they begin to diverge again; and, as the wax is withdrawn still farther, they continue to open, in consequence of the electricity in the fog, till they reach their original distance from each other E. There is very little disturbance by the wind, and the little there is, only wafts them in a small degree, but they keep separate. If they are held near the tiling, or brick-work, of a neighbouring house, they close, F; but begin to diverge again, at the distance of 3 or 4 feet from it, G; and their divergence increases, as they recede from the building, till they separate $\frac{1}{2}$ or $\frac{3}{4}$ inch, as at first, H.

M E M O R A N D U M.

October 3, 1771, I tried the electricity of a thick fog, and (in at least twenty different trials) found the balls separated from $\frac{1}{4}$ to $\frac{3}{4}$ inch distance. Whenever I brought them near the building, or approached them with a stick of excited wax, they closed ; and opened again, on removing them.

W. Henly.

XIX. *Observations on different Kinds of Air.* By Joseph Priestley, LL. D.
F. R. S.

Read March 5, 12, 19, 26, 1772.

THE following observations on the properties of several different kinds of air, I am sensible, are very imperfect, and some of the courses of experiments are incomplete; but a considerable number of facts, which appear to me to be new and important, are sufficiently ascertained; and I am willing to hope, that when philosophers in general are apprized of them, some persons may be able to pursue them to more advantage than myself. I therefore think it my duty to give this Society an account of the progress I have been able to make; and I shall not fail to communicate any farther lights that may occur to me, whenever I resume these inquiries.

In writing upon this subject, I find myself at a loss for proper terms, by which to distinguish the different kinds of air. Those which have hitherto obtained are by no means sufficiently characteristic, or distinct. The terms in common use are, fixed air, mephitic, and inflammable. The last, indeed, sufficiently characterizes and distinguishes that kind of air which takes fire, and explodes on the approach of flame; but it might have been termed fixed with

as much propriety as that to which Dr. Black and others have given that denomination, since it is originally part of some solid substance, and exists in an unelastic state, and therefore may be also called factitious. The term mephitic is equally applicable to what is called fixed air, to that which is inflammable, and to many other kinds; since they are equally noxious, when breathed by animals. Rather, however, than to introduce new terms, or change the signification of old ones, I shall use the term fixed air, in the sense in which it is now commonly used, and distinguish the other kinds by their properties, or some other periphrasis. I shall be under a necessity, however, of giving a name to one species of air, to which no name was given before.

OF FIXED AIR.

Fixed air is that which is expelled by heat from lime, and other calcareous substances, and, when deprived of which, they become quick-lime. It is also contained in alkaline salts, and is generated in great quantities from fermenting vegetables; and being united with water, gives it the principal properties of Pyrmont-water. This kind of air is also well known to be fatal to animals; and Dr. Macbride has demonstrated, that it checks or prevents putrefaction.

Living for some time in the neighbourhood of a public brewery, I was induced to make a few experiments on this kind of air, there being always a large body of it, ready formed, upon the surface of the fermenting liquor, generally about nine inches
or

or a foot in depth, within which any kind of substance may be very conveniently placed; and though it must be continually mixing with the common air, and is far from being perfectly pure, yet there is a constant supply from the fermenting liquor, and it is pure enough for many purposes.

A person, who is quite a stranger to the properties of this kind of air, would be agreeably amused with extinguishing lighted candles, or chips of wood in it, as it lies upon the surface of the fermenting liquor; for the smoke readily unites with this kind of air, probably by means of the water which it contains; so that very little or none of the smoke will escape into the open air, which is incumbent upon it. It is remarkable, that the upper surface of this smoke, floating in the fixed air, is smooth, and well defined; whereas the lower surface is exceedingly ragged, several parts hanging down to a considerable distance within the body of the fixed air, and sometimes in the form of balls, connected to the upper stratum by slender threads, as if they were suspended. The smoke is also apt to form itself into broad flakes, parallel to the surface of the liquor, and at different distances from it, exactly like clouds. These appearances will sometimes continue above an hour, with very little variation. When this fixed air is very strong, the smoke of a small quantity of gunpowder fired in it will be wholly retained by it, no part escaping into the common air.

Making an agitation in this air, the surface of it, which still continues to be exactly defined, is thrown into the form of waves, which it is very amusing to look upon; and if, by this agitation, any of the fixed
air

air be thrown over the side of the vessel, the smoke, which is mixed with it, will fall to the ground, as if it was so much water, the fixed air being heavier than common air.

The red part of burning wood was extinguished in this air, but I could not perceive that a red-hot poker was sooner cooled in it.

Fixed air does not instantly mix with common air. Indeed, if it did, it could not be caught upon the fermenting liquor; for a candle put under a large receiver, and immediately plunged very deep below the surface of the fixed air, will burn some time. But vessels with the smallest orifices, hanging with their mouths downwards in the fixed air, will in time have the common air, which they contain, perfectly mixed with it. When the fermenting liquor is contained in vessels close covered up, the fixed air is rendered much stronger, and then it readily affects the common air which is contiguous to it; so that, upon removing the cover, candles held at a considerable distance above the surface will instantly go out. I have been told by the workmen, that this will sometimes be the case, when the candles are held more than half a yard above the mouth of the vessel.

Fixed air unites with the smoke of resin, sulphur, and other electrical substances, as well as with the vapour of water; and yet, by holding the wire of a charged phial among these fumes, I could not make any electrical atmosphere, which surprized me a good deal, as there was a large body of this smoke, and it was so confined, that it could not escape me. I also held some oil of vitriol in a glass vessel within the

the fixed air, and by plunging a piece of red hot glass into it, raised a copious and thick fume. This floated upon the surface of the fixed air like other fumes, and continued as long.

Considering the near affinity between water and fixed air, I concluded that if a quantity of water was placed near the yeast of the fermenting liquor, it could not fail to imbibe that air, and thereby acquire the principal properties of Pyrmont, and other medicinal mineral waters. Accordingly, I found, that when the surface of the water was considerable, it always acquired the pleasant acidulous taste that Pyrmont water has. The readiest way of impregnating water with this virtue, in these circumstances, is to take two vessels, and to keep pouring the water from one into the other; when they are both of them held as near the yeast as possible; for by this means a great quantity of surface is exposed to the air, and the surface is also continually changing. In this manner, I have sometimes, in the space of two or three minutes, made a glass of exceedingly pleasant sparkling water, which could hardly be distinguished from very good Pyrmont.

But the most effectual way of impregnating water with fixed air is to put the vessels which contain the water into glass jars, filled with the purest fixed air, made by the solution of chalk in diluted oil of vitriol, standing in quicksilver. In this manner I have, in about two days, made a quantity of water to imbibe more than an equal bulk of fixed air, so that, according to Dr. Brownrigg's experiments, it must have been much stronger than the best imported Pyrmont; for though he made his experiments at the spring head

head, he never found that it contained quite so much as half its bulk of this air. If a sufficient quantity of quicksilver cannot be procured, oil may be used with sufficient advantage, for this purpose, as it imbibes the fixed air very slowly. Fixed air may be kept in vessels standing in water for a long time, if they be separated by a partition of oil, about half an inch thick. Pyrmont water made in these circumstances, is little or nothing inferior to that which has stood in quicksilver.

The *readiest* method of preparing this water for use is to agitate it strongly with its whole surface exposed to the fixed air. By this means also, more than an equal bulk of air may be communicated to a large quantity of water in the space of a few minutes. Easy directions for doing this I have published in a small pamphlet, designed originally for the use of seamen in long voyages, on the presumption that it might be of use for preventing or curing the sea scurvy, equally with wort, which was recommended by Dr. Macbride for this purpose, on no other account than its property of generating fixed air, by its fermentation in the stomach.

Water thus impregnated with fixed air readily dissolves iron, as Mr. Lane has discovered; so that if a quantity of iron filings be put to it, it presently becomes a strong chalybeate, and of the mildest and most agreeable kind.

I have recommended the use of chalk and oil of vitriol as the cheapest, and, upon the whole, the best materials for this purpose; and whereas some persons had suspected that a quantity of the oil of vitriol was rendered volatile by this process, I examined it
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by all the chemical methods that are in use; but could not find that water thus impregnated contained the least perceivable quantity of the acid.

Mr. Hey, indeed, who assisted me in this examination, found that distilled water, impregnated with fixed air, did not mix so readily with soap as the distilled water itself; but this was also the case when the fixed air had passed through a long glass tube filled with alkaline salts, which, it may be supposed, would have imbibed any of the oil of vitriol that might have been contained in that air*.

It is not improbable but that fixed air itself may be of the nature of an acid, though of a weak and peculiar sort. Mr. Bergman of Upsal, who honoured me with a letter upon the subject, calls it the aërial acid, and, among other experiments to prove it to be an acid, he says that it changes the blue juice of tournesole into red.

The heat of boiling water will expell all the fixed air, if a phial containing the impregnated water be held in it; but it will often require above half an hour to do it completely.

Dr. Percival, who is particularly attentive to every improvement in the medical art, and who has thought so well of this impregnation as to prescribe it in several cases, informs me that it seems to be much stronger, and sparkles more, like the true Pyrmont water, after it has been kept some time. This circumstance, however, shews that, in time, the fixed air is more easily disengaged from the water, and

* An account of Mr. Hey's experiments will be found in the Appendix to these papers.

though, in this state, it may affect the taste more sensibly, it cannot be of so much use in the stomach and bowels, as when the air is more firmly retained by the water, though, in consequence of it, it be less sensible to the taste.

By the process described in my pamphlet, fixed air may be readily incorporated with wine, beer, and almost any other liquor whatever; and when beer, wine, or cyder, is become flat or dead (which is the consequence of the escape of the fixed air they contained), they may be revived by this means; but the delicate and agreeable flavour, or acidulous taste, communicated by fixed air, and which is very manifest in water, can hardly be perceived in wine, or any liquors which have much taste of their own.

I should think that there can be no doubt, but that water thus impregnated with fixed air must have all the medicinal virtues of genuine Pyrmont water; since these depend upon the fixed air it contains. If the genuine Pyrmont water derives any advantage from its being a natural chalybeate, this may also be obtained by providing a common chalybeate water, and using it in these processes, instead of common water.

Having succeeded so well with this artificial Pyrmont water, I imagined that it might be possible to give ice the same virtue, especially as cold is known to promote the absorption of fixed air by water; but in this I found myself quite mistaken. I put several pieces of ice into a quantity of fixed air, confined by quicksilver, but no part of the air was absorbed in two days and two nights; but upon bringing it into a place where the ice melted, the air

was absorbed as usual. I then took a quantity of strong artificial Pyrmont water, and, putting it into a thin glass phial, I set it in a pot that was filled with snow and salt. This mixture instantly freezing the water that was contiguous to the sides of the glass, the air was discharged plentifully, so that I caught a considerable quantity, in a bladder tied to the mouth of the phial. I also took two quantities of the same Pyrmont water, and placed one of them where it might freeze, keeping the other in a cold place, but where it would not freeze. This retained its acidulous taste, though the phial which contained it was not corked; whereas the other, being brought into the same place, where the ice melted very slowly, had at the same time the taste of common water only. That quantity of water which had been frozen by the mixture of snow and salt, was almost as much like snow as ice, such a quantity of air bubbles were contained in it, by which it was prodigiously increased in bulk.

The pressure of the atmosphere assists very considerably in keeping fixed air confined in water; for in an exhausted receiver, Pyrmont water will absolutely boil, by the copious discharge of its air. This is also the reason why beer and ale froth so much *in vacuo*. I do not doubt, therefore, but that, by the help of a condensing engine, water might be much more highly impregnated with the virtues of the Pyrmont spring, and it would not be difficult to contrive a method of doing it.

The manner in which I made several experiments to ascertain the absorption of fixed air by different fluid substances was to put the liquid into a dish,

and holding it within the body of the fixed air at the brewery, to set a glass vessel into it, with its mouth inverted. This glass being necessarily filled with the fixed air, the liquor would rise into it when they were both taken into the common air, if the fixed air was absorbed at all.

Making use of ether in this manner, there was a constant bubbling from under the glass, occasioned by this fluid easily rising in vapour, so that I could not, in this method, determine whether it imbibed the air or not. I concluded, however, that they did incorporate, from a very disagreeable circumstance, which made me desist from making any more experiments of the kind. For all the beer, over which this experiment was made, contracted a peculiar taste, the fixed air impregnated with the ether being, I suppose, again absorbed by the beer. I have also observed, that water which remained a long time within this air has sometimes acquired a very disagreeable taste. At one time it was like tar-water. How this was acquired, I was very desirous of making some experiments to ascertain, but I was discouraged by the fear of injuring the fermenting liquor. It could not come from the fixed air only.

Having imagined that fixed air coagulated the blood in the lungs of animals, and thereby caused instant death; I suffocated a cat in this kind of air, and examining the lungs presently after, found them collapsed and white, having little or no blood in them.

In order to try the effect of this air upon the blood itself, I took a quantity from a fowl just killed, and divided it into two parts, holding one of them within the
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the fixed air, and the other in the common air, and observed that the former was coagulated much sooner than the latter. This I could wish to have tried again.

Insects and animals which breathe very little are stifled in fixed air, but are not soon quite killed in it. Butterflies, and flies of other kinds, will generally become torpid, and seemingly dead, after being held a few minutes over the fermenting liquor; but they revive again after being brought into the fresh air. But there are very great varieties with respect to the time in which different kinds of flies will either become torpid in the fixed air, or die in it. A large strong frog was much swelled, and seemed to be nearly dead, after being held about six minutes over the fermenting liquor; but it recovered upon being brought into the common air. A snail treated in the same manner died presently.

Fixed air is presently fatal to vegetable life. At least sprigs of mint, growing in water, and placed over the fermenting liquor, will often become quite dead in one day, or even in a less space of time; nor do they recover when they are afterwards brought into the common air. I am told, however, that some other plants are much more hardy in this respect.

A red rose, fresh gathered, lost its redness, and became of a purple colour, after being held over the fermenting liquor about twenty-four hours; but the tips of each leaf were much more affected than the rest of it. Another red rose turned perfectly white in this situation; but various other flowers, of different colours, were very little affected. These experiments

riments were not repeated, as I wish they might be done, in pure fixed air, extracted from chalk by means of oil of vitriol.

For every purpose, in which it was necessary that the fixed air should be as unmixed as possible, I generally made it by pouring oil of vitriol upon chalk and water, catching it in a bladder, fastened to the neck of the phial, in which they were contained, taking care to press out all the common air, and also the first, and sometimes the second, produce of fixed air; and also, by agitation, making it as quickly as I possibly could. At other times, I made it pass from the phial in which it was generated through a glass tube, without the intervention of any bladder, which, as I found by experience, will not long make a sufficient separation between several kinds of air and common air.

I had once thought that the readiest method of procuring fixed air, and in sufficient purity, would be by the simple process of burning chalk, or pounded lime-stone in a gun-barrel, making it pass through the stem of a tobacco-pipe, or a glass tube carefully luted to the orifice of it; and in this manner I find that air is produced in great plenty; but, upon examining it, I found, to my very great surprize, that little more than one half of it was fixed air, capable of being absorbed by water; and that the rest was inflammable, sometimes very weakly, but sometimes pretty highly so. Whence this inflammability proceeds, I am not able to determine, the lime or chalk not being supposed to contain any other than fixed air. I conjecture, however, that it must proceed from the iron, and the separation of it
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from the calx may be promoted by that small quantity of oil of vitriol, which I am informed is contained in chalk, if not in lime-stone also. But it is an objection to this hypothesis, that the inflammable air produced in this manner burns blue, and not at all like that which is produced from iron, or any other metal, by means of an acid. It has also the smell of that kind of inflammable air which is produced from vegetable substances. Besides, oil of vitriol without water, will not dissolve iron; nor can inflammable air be got from it, unless the acid be considerably diluted; and when I mixed brimstone with the chalk, neither the quality nor the quantity of the air was changed by it. Indeed no air, or permanently elastic vapour, can be got from brimstone, or any oil.

In the method in which I generally made the fixed air, and indeed always, unless the contrary be particularly mentioned, *viz.* by diluted oil of vitriol and chalk, I found by experiment that it was as pure as Mr. Cavendish made it. For after it had passed through a large body of water in small bubbles, still $\frac{1}{30}$ or $\frac{1}{60}$ part only was not absorbed by water. In order to try this as expeditiously as possible, I kept pouring the air from one glass vessel into another, immersed in a quantity of cold water, in which manner I found by experience, that almost any quantity may be reduced as far as possible in little more than a quarter of an hour.

At the same time that I was trying the purity of my fixed air, I had the curiosity to endeavour to ascertain whether that part of it which is not miscible in water, be equally diffused through the whole mass;

mass; and, for this purpose, I divided a quantity of about a gallon into three parts, the first consisting of that which was uppermost, and the last of that which was the lowest, contiguous to the water; but all these parts were reduced in about an equal proportion, by passing through the water, so that the whole mass had been of an uniform composition. This I have also found to be the case with several kinds of air, which will not properly incorporate.

A mouse will live very well, though a candle will not burn, in the residuum of the purest fixed air that I can make; and I once made a very large quantity for the sole purpose of this experiment. This, therefore, seems to be one instance of the generation of genuine common air, though vitiated in some degree. It is also another proof of the residuum of fixed air being, in part at least, common air, that it becomes turbid, and is diminished by the mixture of nitrous air, as will be explained hereafter.

That fixed air only wants some addition to make it permanent, and immiscible with water, if not, in all respects, common air, I have been led to conclude, from several attempts which I once made to mix it with air, in which a quantity of iron filings and brimstone, made into a paste with water, had stood; for, in several mixtures of this kind, I imagined that not much more than half of the fixed air could be imbibed by water; but, not being able to repeat the experiment, I conclude that I either deceived myself in it, or that I overlooked some circumstance on which the success of it depended.

These experiments, however, whether they were fallacious or otherwise, induced me to try whether
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any alteration would be made in the constitution of fixed air, by this mixture of iron filings and brimstone. I therefore put a mixture of this kind into a quantity of as pure fixed air as I could make, and confined the whole in quicksilver, lest the water should absorb it before the effects of the mixture could take place. The consequence was, that the fixed air was diminished, and the quicksilver rose in the vessel, till about the fifth part was occupied by it; and, as near as I could judge, the process went on, in all respects, as if the air in the inside had been common air.

What is most remarkable, in the result of this experiment, is, that the fixed air, into which this mixture had been put, and which had been in part diminished by it, was in part also rendered insoluble in water by this means. I made this experiment four times, with the greatest care, and observed, that in two of them about one sixth, and in the other two about one fourteenth, of the original quantity, was such as could not be absorbed by water, but continued permanently elastic. Lest I should have made any mistake with respect to the purity of the fixed air, the last time that I made the experiment, I set part of the fixed air, which I made use of, in a separate vessel, and found it to be exceedingly pure, so as to be almost wholly absorbed by water; whereas the other part, to which I had put the mixture, was far from being so.

In one of these cases, in which fixed air was made immiscible with water, it appeared to be not very noxious to animals; but in another case, a mouse died in it pretty soon.

As the iron is reduced to a calx by this process, I once concluded, that it is phlogiston that fixed air wants, to make it common air; and, for any thing I yet know, this may be the case, though I am ignorant of the method of combining them; and when I calcined a quantity of lead in fixed air, in the manner which will be described hereafter, it did not seem to have been less soluble in water than it was before.

II.

ON AIR IN WHICH A CANDLE, OR BRIMSTONE, HAS BURNED OUT.

It is well known that flame cannot subsist long without change of air, so that the common air is necessary to it, except in the case of substances, into the composition of which nitre enters; for these will burn *in vacuo*, in fixed air, and even under water, as is evident in some rockets, which are made for this purpose. The quantity of air which even a small flame requires to keep it burning is prodigious. It is generally said, that an ordinary candle consumes, as it is called, about a gallon in a minute. Considering this amazing consumption of air, by fires of all kinds, volcano's, &c. it becomes a great object of philosophical inquiry, to ascertain what change is made in the constitution of the air by flame, and to discover what provision there is in nature for remedying the injury which the atmosphere receives by this means. Some of the following experiments will, perhaps, be thought to throw a little light upon the subject.

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The diminution of the quantity of air in which a candle, or brimstone, has burned out, is various; but I imagine that, at a medium, it may be about one fifteenth, or one sixteenth, of the whole; about one third as much as by animals breathing it as long as they can, by animal or vegetable substances putrifying in it, by the calcination of metals, or by a mixture of steel filings and pounded brimstone standing in it.

I have sometimes thought, that flame disposes the common air to deposit the fixed air it contains; for if any lime-water be exposed to it, it immediately becomes turbid. This is the case, when wax candles, tallow candles, chips of wood, spirit of wine, æther, and every other substance which I have yet tried, except brimstone, is burned in a close glass vessel, standing in lime-water. This precipitation of fixed air (if this be the case) may be owing to something emitted from the burning bodies, which has a stronger affinity with the other constituent parts of the atmosphere.

If brimstone be burned in the same circumstances, the lime-water continues transparent, but still there may have been the same precipitation of the fixed part of the air; but that, uniting with the lime and the vitriolic acid, it forms a selenetic salt, which is soluble in water. Having evaporated a quantity of water thus impregnated, by burning brimstone a great number of times over it, a whitish powder remained, which had an acid taste; but repeating the experiment with a quicker evaporation, the powder had no acidity; but was very much like chalk. The burning of brimstone but once over a

quantity of lime-water, will affect it in such a manner, that breathing into it will not make it turbid, which otherwise it always presently does.

Dr. Hales supposed, that by burning brimstone repeatedly in the same quantity of air, the diminution would continue without end. But this I have frequently tried, and not found to be the case. Indeed, when the ignition has been imperfect in the first instance, a second firing of the same substance will increase the effect of the first, &c. but this progress soon ceases. In many cases of the diminution of air, the effect is not immediately apparent, even when it stands in water; for sometimes the bulk of air will not be much reduced, till it has passed several times through a quantity of water, which has thereby a better opportunity of absorbing that fluid part of the air, which had not been perfectly detached from the rest. I have sometimes found a very great reduction of a mass of air, in consequence of passing but once thorough cold water. If the air has stood in quicksilver, the diminution is generally inconsiderable, till it has undergone this operation, there not being any substance exposed to the air that could absorb any part of it.

I could not find any considerable alteration in the specific gravity of the air, in which candles, or brimstone, had burned out. I am satisfied, however, that it is not heavier than common air, which must have been manifest, if so great a diminution of the quantity had been owing, as Dr. Hales and others supposed, to the elasticity of the whole mass being impaired. After making several trials for this purpose, I concluded that air, thus diminished in bulk,
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is rather lighter than common air, which favours the supposition of the fixed, or heavier part of the common air, having been precipitated.

An animal will live nearly, if not quite as long, in air in which candles have burned out, as in common air. This fact surprized me very greatly, having imagined that what is called the consumption of air by flame, or respiration, to have been of the same nature; but I have since found, that this fact has been observed by many persons, and even so early as by Mr. Boyle. I have also observed, that air in which brimstone has burned, is not in the least injurious to animals, after the fumes, which at first make it very cloudy, have intirely subsided.

Having read, in the Memoirs of the Society at Turin, Vol. I. p. 41. that air in which candles had burned out was perfectly restored, so that other candles would burn in it again as well as ever, after having been exposed to a considerable degree of cold, and likewise after having been compressed in bladders (for the cold had been supposed to have produced this effect by nothing but condensation): I repeated these experiments, and did, indeed, find, that, when I compressed the air in bladders, as the Count de Saluce, who made the observation, had done, the experiment succeeded: but having had sufficient reason to distrust bladders, I compressed the air in a glass vessel standing in water; and then I found, that this process is altogether ineffectual for the purpose. I kept the air compressed much more, and much longer, than he had done, but without producing any alteration in it. I also find, that a greater degree of cold than that which he applied, and
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of longer continuance, did by no means restore this kind of air: for when I have exposed the phials which contained it a whole night, in which the frost was very intense; and also when I kept it surrounded with a mixture of snow and salt, I found it, in all respects, the same as before.

It is also advanced, in the same Memoir, p. 41. that heat only, as the reverse of cold, renders air unfit for candles burning in it. But I repeated the experiment of the Count for that purpose, without finding any such effect from it. I also remember that, many years ago, I filled an exhausted receiver with air, that had passed through a glass tube made red-hot, and found that a candle would burn in it perfectly well. Also, rarefaction by the air-pump does not injure air in the least degree.

Though this experiment failed, I flatter myself that I have accidentally hit upon a method of restoring air which has been injured by the burning of candles, and that I have discovered at least one of the restoratives which nature employs for this purpose. It is vegetation. In what manner this process in nature operates, to produce so remarkable an effect, I do not pretend to have discovered; but a number of facts declare in favour of this hypothesis. I shall introduce my account of them, by reciting some of the observations which I made on the growing of plants in confined air, which led to this discovery.

One might have imagined that, since common air is necessary to vegetable, as well as to animal life, both plants and animals had affected it in the same manner, and I own I had that expectation, when

when I first put a sprig of mint into a glass-jar, standing inverted in a vessel of water; but when it had continued growing there for some months, I found that the air would neither extinguish a candle, nor was it at all inconvenient to a mouse, which I put into it.

The plant was not affected any otherwise than was the necessary consequence of its confined situation; for plants growing in several other kinds of air, were all affected in the very same manner. Every succession of leaves was more diminished in size than the preceding, till, at length, they came to be no bigger than the heads of pins. The root decayed, and the stalk also, beginning from the root; and yet the plant continued to grow upwards, drawing its nourishment through a black and rotten stem. In the third or fourth set of leaves, long hairy filaments grew from the insertion of each leaf, and sometimes from the body of the stem, shooting out as far as the vessel in which it grew would permit, which, in my experiments, was about two inches. In this manner a sprig of mint lived, the old stem decaying, and new ones shooting up in its place, but less and less continually, all the summer season.

In repeating this experiment, care must be taken to draw away all the dead leaves from about the plant, lest they should putrefy, and affect the air. I have found that a fresh cabbage leaf, put under a glass vessel filled with common air, for the space of one night only, has so far affected the air, that a candle would not burn in it the next morning, and yet the leaf had not acquired any smell of putrefaction.

Finding that candles burn very well in air in which plants had grown a long time, and having had some reason to think, that there was something attending vegetation, which restored air that had been injured by respiration, I thought it was possible that the same process might also restore the air that had been injured by the burning of candles.

Accordingly, on the 17th of August, 1771, I put a sprig of mint into a quantity of air, in which a wax candle had burned out, and found that, on the 27th of the same month, another candle burned perfectly well in it. This experiment I repeated, without the least variation in the event, not less than eight or ten times in the remainder of the summer. Several times I divided the quantity of air in which the candle had burned out, into two parts, and putting the plant into one of them, left the other in the same exposure, contained, also, in a glass vessel immersed in water, but without any plant; and never failed to find, that a candle would burn in the former, but not in the latter. I generally found that five or six days were sufficient to restore this air, when the plant was in its vigour; whereas I have kept this kind of air in glass vessels, immersed in water many months, without being able to perceive that the least alteration had been made in it. I have also tried a great variety of experiments upon it, as by condensing, rarefying, exposing to the light and heat, &c. and throwing into it the effluvia of many different substances, but without any effect.

Experiments made in the year 1772, abundantly confirmed my conclusion concerning the restoration of air, in which candles had burned out by plants
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growing in it. The first of these experiments was made in the month of May; and they were frequently repeated in that and the two following months, without a single failure.

For this purpose I used the flames of different substances, though I generally used wax or tallow candles. On the 24th of June the experiment succeeded perfectly well with air in which spirit of wine had burned out, and on the 27th of the same month it succeeded equally well with air in which brimstone matches had burned out, an effect of which I had despaired the preceding year.

This restoration of air I found depended upon the vegetating state of the plant; for though I kept a great number of the fresh leaves of mint in a small quantity of air in which candles had burned out, and changed them frequently, for a long space of time, I could perceive no melioration in the state of the air.

This remarkable effect does not depend upon any thing peculiar to mint, which was the plant that I always made use of till July 1772; for on the 16th of that month, I found a quantity of this kind of air to be perfectly restored by sprigs of balm, which had grown in it from the 7th of the same month.

That this restoration of air was not owing to any aromatic effluvia of these two plants, not only appeared by the essential oil of mint having no sensible effect of this kind; but from the equally complete restoration of this vitiated air by the plant called groundsel, which is usually ranked among the weeds, and has an offensive smell. This was the result of an experiment made the 16th of July, when the

plant had been growing in the burned air from the 8th of the same month. Besides, the plant which I have found to be the most effectual of any that I have tried for this purpose is spinach, which is of quick growth, but will seldom thrive long in water. One jar of burned air was perfectly restored by this plant in four days, and another in two days. This last was observed on the 22d of July. In general this effect may be presumed to have taken place in much less time than I have mentioned; because I never chose to make a trial of the air, till I was pretty sure, from preceding observations, that the event which I had expected must have taken place, if it would succeed at all; lest, returning back that part of the air on which I made the trial, and which would thereby necessarily receive a small mixture of common air, the experiment might not be judged to be quite fair; though I myself might be sufficiently satisfied with respect to the allowance that was to be made for that small imperfection.

III.

OF INFLAMMABLE AIR.

I have generally made inflammable air in the manner described by Mr. Cavendish, in the Philosophical Transactions, from iron, zinc, or tin; but chiefly from the two former metals, on account of the process being the least troublesome: but when I extracted it from vegetable or animal substances, or from coals, I put them into a gun barrel, to the office of which I joined a glass tube, or the stem of a to-

a tobacco pipe, and to the end of this I tied a flaccid bladder, in order to catch the generated air.

There is not, I believe, any vegetable or animal substance whatever, nor any mineral substance, that is inflammable, but what will yield great plenty of inflammable air, when they are treated in this manner, and urged with a strong heat; but, in order to get the most air, the heat must be applied as suddenly, and as vehemently, as possible. For, notwithstanding the same care be taken in luting, and in every other respect, six or even ten times more air may be got by a sudden heat than by a slow one, though the heat that is last applied be as intense as that which was applied suddenly. A bit of dry oak, weighing about twelve grains, will generally yield about a sheep's bladder full of inflammable air with a brisk heat, when it will only give about two or three ounce measures if the same heat be applied to it very gradually. To what this difference is owing, I cannot tell.

Inflammable air, when it is made by a quick process, has a very strong and offensive smell, from whatever substance it be generated; but this smell is of three different kinds, according as the air is extracted from mineral, vegetable, or animal substances. The last is exceedingly fetid; and it makes no difference, whether it be extracted from a bone, or even an old and dry tooth, or from soft muscular flesh, or any other part of the animal. The burning of any substance occasions the same smell: for the gross fume which arises from them, before they flame, is the inflammable air they contain, which is expelled by heat, and then readily ignited. The smell of in-

flammable air is the very same, as far as I am able to perceive, from whatever substance of the same kingdom it be extracted. Thus it makes no difference whether it be got from iron, zinc, or tin, from any kind of wood, or, as was observed before, from any part of an animal.

If a quantity of inflammable air be contained in a glass vessel standing in water, and have been generated very fast, it will smell even through the water, and this water will also soon become covered with a thin film, assuming all the different colours. If the inflammable air have been generated from iron, this matter will appear to be a red okre, or the earth of iron, as I have found by collecting a considerable quantity of it; and if it have been generated from zinc, it is a whitish substance, which I suppose to be the calx of the metal. It likewise settles to the bottom of the vessel, and when the water is stirred, it has very much the appearance of wool. When water is once impregnated in this manner, it will continue to yield this scum for a considerable time after the air is removed from it. This I have often observed with respect to iron.

Inflammable air, made by a violent effervescence, I have observed to be much more inflammable than that which is made by a weak effervescence, whether the water or the oil of vitriol prevailed in the mixture. Also the offensive smell was much stronger in the former case than in the latter. The greater degree of inflammability appeared by the greater number of successive explosions, when a candle was presented to the neck of a phial filled with it. It is possible, however, that this diminution of inflammability

flammability may, in some measure, arise from the air continuing so much longer in the bladder when it is made very slowly; though I think the difference is too great for this cause to have produced the whole of it. It may, perhaps, deserve to be tried by a different process, without a bladder.

Inflammable air is not thought to be miscible with water, and when kept many months, seems, in general, to be as inflammable as ever. Indeed, when it is extracted from vegetable or animal substances, a part of it will be imbibed by the water in which it stands; but it may be presumed, that in this case, there was a mixture of fixed air extracted from the substance along with it. I have indisputable evidence, however, that inflammable air, standing long in water, has actually lost all its inflammability, and even come to extinguish flame much more than that air in which candles have burned out. After this change it appears to be greatly diminished in quantity, and it still continues to kill animals the moment they are put into it.

This very remarkable fact first occurred to my observation on the twenty-fifth of May 1771, when I was examining a quantity of inflammable air, which had been made from zinc, near three years before. Upon this, I immediately set by a common quart bottle filled with inflammable air from iron, and another equal quantity from zinc; and examining them in the beginning of December following, that from the iron was reduced near one half in quantity, if I be not greatly mistaken; for I found the bottle half full of water, and I am pretty clear that it was full of air when it was set by. That which had
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been produced from zinc was not altered, and filled the bottle as at first.

Another instance of this kind occurred to my observation on the 19th of June 1772, when a quantity of air, half of which had been inflammable air from zinc, and half air in which mice had died, and which had been put together the 30th of July 1771, appeared not to be in the least inflammable, but extinguished flame, as much as any kind of air that I had ever tried. I think that, in all, I have had four instances of inflammable air losing its inflammability, while it stood in water.

Though air tainted with putrefaction extinguishes flame, I have not found that animals or vegetables putrefying in inflammable air render it less inflammable. But one quantity of inflammable air, which I had set by in May 1771, along with the others above mentioned, had had some putrid flesh in it; and this air had lost its inflammability, when it was examined at the same time with the other in the December following. The bottle in which this air had been kept, smelled exactly like very strong Harrowgate water. I do not think that any person could have distinguished them.

I have made plants grow for several months in inflammable air made from zinc, and also from oak; but, though the plants grew pretty well, the air still continued inflammable. The former, indeed, was not so highly inflammable as when it was fresh made, but the latter was quite as much so; and the diminution of inflammability in the former case, I attribute to some other cause than the growth of the plants.

No kind of air, on which I have yet made the experiment, will conduct electricity; but the colour of a spark is remarkably different in some different kinds of air, which seems to shew that they are not equally good non-conductors. In fixed air, the electric spark is exceedingly white; but in inflammable air it is of a purple, or red colour. Now, since the most vigorous sparks are always the whitest, and, in other cases, when the spark is red, there is reason to think that the electric matter passes with difficulty, and with less rapidity: it is possible that the inflammable air may contain particles which conduct electricity, though very imperfectly; and that the whiteness of the spark in the fixed air, may be owing to its meeting with no conducting particles at all. When an explosion was made in a quantity of inflammable air, it was a little white in the center, but the edges of it were still tinged with a beautiful purple. The degree of whiteness in this case was probably owing to the electric matter rushing with more violence in an explosion than in a common spark.

Inflammable air kills animals as suddenly as fixed air, and, as far as can be perceived, in the same manner, throwing them into convulsions, and thereby occasioning present death. I had imagined that, by animals dying in a quantity of inflammable air, it would in time become less noxious; but this did not appear to be the case; for I killed a great number of mice in a small quantity of this air, which I kept several months for this purpose, without its being at all sensibly mended; the last, as well as the first mouse, dying the moment it was put into it.

I once imagined that, since fixed and inflammable air are the reverse of one another, in several remarkable properties, a mixture of them would make common air; and while I made the mixtures in bladders, I imagined that I had succeeded in my attempt; but I have since found that thin bladders do not sufficiently prevent the air that is contained in them from mixing with the external air. Also corks will not sufficiently confine different kinds of air, unless the phials in which they are confined be set with their mouths downwards, and a little water lie in the necks of them, which, indeed, is equivalent to the air standing in vessels immersed in water. In this manner, however, I have kept different kinds of air for several years.

Whatever methods I took to promote the mixture of fixed and inflammable air, they were all ineffectual. I think it my duty, however, to recite the issue of an experiment or two of this kind, in which equal mixtures of these two kinds of air had stood near three years, as they seem to shew that they had in part affected one another, in that long space of time. These mixtures I examined April 27, 1771. One of them had stood in quicksilver, and the other in a corked phial, with a little water in it. On opening the latter in water, the water instantly rushed in, and filled almost half of the phial, and very little more was absorbed afterwards. In this case the water in the phial had probably absorbed a considerable part of the fixed air, so that the inflammable air was exceedingly rarefied; and yet the whole quantity that must have been rendered non-elastic was ten times more than the bulk of the water; and it has not

not been found that water can contain much more than its own bulk of fixed air. But in other cases I have found the diminution of a quantity of air, and especially of fixed air, to be much greater than I could well account for by any kind of absorption.

The phial which had stood immersed in quicksilver had lost very little of its original quantity; and being now opened in water, and left there, along with a another phial, which was just then filled, as this had been three years before, with air half inflammable and half fixed, I observed that the quantity of both was diminished, by the absorption of the water, in the same proportion.

Upon applying a candle to the mouths of the phials which had been kept three years, that which had stood in quicksilver went off at one explosion, exactly as it would have done if there had been a mixture of common air, with the inflammable. As a good deal depends upon the apertures of the vessels in which the inflammable air is fixed, I mixed the two kinds of air in equal proportion in the same phial, and after letting it stand some days in water, that the fixed air might be absorbed, I applied a candle to it; but it made ten or twelve explosions (stopping the phial after each of them) before the inflammable matter was exhausted.

The air which had been confined in the corked phial exploded in the very same manner as an equal mixture of the two kinds of air in the same phial, the experiment being made as soon as the fixed air was absorbed, as before; so that, in this case, the two kinds of air did not seem to have affected one another at all.

Considering inflammable air as air united to or loaded with phlogiston, I exposed to it several substances, which are said to have a near affinity with phlogiston, as oil of vitriol, and spirit of nitre (the former for above a month), but without making any sensible alteration in it.

I observed, however, that inflammable air, mixed with the fumes of smoaking spirit of nitre, goes off at one explosion, exactly like a mixture of half common and half inflammable air. This I tried several times, by throwing the inflammable air into a phial full of spirit of nitre, with its mouth immersed in a basin containing some of the same spirit, and then applying the flame of a candle to the mouth of the phial, the moment that it was uncovered, after it had been taken out of the basin. This remarkable effect I hastily concluded to have arisen from the inflammable air having been in part deprived of its inflammability, by means of the stronger affinity, which the spirit of nitre had with phlogiston, and therefore I imagined that by letting them stand longer in contact, and especially by agitating them strongly together, I should deprive the air of all its inflammability; but neither of these operations succeeded, for still the air was only exploded at once, as before. And lastly, when I passed a quantity of inflammable air, which had been mixed with the fumes of spirit of nitre, through a body of water, and received it in another vessel, it appeared not to have undergone any change at all, for it went off in several successive explosions, like the purest inflammable air. The effect abovementioned must, therefore, have been owing to the fumes of the spirit of nitre supplying the

the place of common air for the purpose of ignition, which is analogous to other experiments with nitre.

Having had the curiosity, on the 25th of July 1772, to expose a great variety of different kinds of air to water out of which the air it contained had been boiled, without any particular view; the result was, in several respects, altogether unexpected, and led to a variety of new observations on the properties and affinities of several kinds of air with respect to water. Among the rest three fourths of that which was inflammable was absorbed by the water in about two days, and the remainder was inflammable, but weakly so.

Upon this, I began to agitate a quantity of strong inflammable air in a glass jar, standing in a pretty large trough of water, the surface of which was exposed to the common air, and I found that when I had continued the operation about ten minutes, near one fourth of the quantity of air had disappeared; and finding that the remainder made an effervescence with nitrous air, I concluded that it must have become fit for respiration, whereas this kind of air is, at the first, as noxious as any other kind whatever. To ascertain this, I put a mouse into a vessel containing $2\frac{1}{2}$ ounce measures of it, and observed that it lived in it twenty minutes, which is as long as a mouse will generally live in the same quantity of common air. This mouse was even taken out alive, and recovered very well. Still also the air in which it had breathed so long was inflammable, though very weakly so. I have even found it to be so when a mouse has actually died in it.

Inflammable air thus diminished by agitation in water, makes but one explosion on the approach of a candle exactly like a mixture of inflammable air with common air.

From this experiment I concluded that, by continuing the same process, I should deprive inflammable air of all its inflammability, and this I found to be the case; for, after a longer agitation, it admitted a candle to burn in it, like common air, only more faintly; and indeed by the test of nitrous air it did not appear to be near so good as common air. Continuing the same process still farther, the air which had been most strongly inflammable a little before, came to extinguish a candle, exactly like air in which a candle had burned out, nor could they be distinguished by the test of nitrous air.

I found, by repeated trials, that it was difficult to catch the time in which inflammable air obtained from metals, in coming to extinguish flame, was in the state of common air, so that the transition from the one to the other must be very short. I readily, however, found this state in a quantity of inflammable air extracted from oak, which air I had kept by me a year, and in which a plant had grown, though very poorly, for some part of the time. A quantity of this air, after being agitated in water till it was diminished about one half, admitted a candle to burn in it exceedingly well, and was even hardly to be distinguished from common air by the test of nitrous air.

I took some pains to ascertain the quantity of diminution, in fresh made and very highly inflammable air from iron, at which it ceased to be inflammable,

mable, and, upon the whole, I concluded that it was so when it was diminished a little more than one half: for a quantity which was diminished exactly one half had something inflammable in it, but in the slightest degree imaginable.

Finding that water would imbibe inflammable air, I endeavoured to impregnate water with it, by the same process by which I had made water imbibe fixed air; but though I found that distilled water would imbibe about one fourteenth of its bulk of inflammable air, I could not perceive that the taste of it was sensibly altered.

IV.

OF AIR INFECTED WITH ANIMAL RESPIRATION; OR PUTREFACTION.

That candles will burn only a certain time, is a fact not better known, than it is that animals can live only a certain time, in a given quantity of air; but the cause of the death of the animal is not better known than that of the extinction of flame in the same circumstances; and when once any quantity of air has been rendered noxious by animals breathing in it as long as they could, I do not know that any methods have been discovered of rendering it fit for breathing again. It is evident, however, that there must be some provision in nature for this purpose, as well as for that of rendering the air fit for sustaining flame; for without it the whole mass of the atmosphere would, in time, become unfit for the purpose of animal life; and yet there is no reason to think that it is, at present, at all less fit for respiration than it

it has ever been. I flatter myself, however, that I have hit upon two of the methods employed by nature for this great purpose. How many others there may be, I cannot tell.

When animals die upon being put into air in which other animals have died, after breathing in it as long as they could, it is plain that the cause of their death is not the want of any *pabulum vitæ*, which has been supposed to be contained in the air, but on account of the air being impregnated with something stimulating to their lungs; for they almost always die in convulsions, and are sometimes affected so suddenly, that they are irrecoverable after a single inspiration, though they be withdrawn immediately, and every method has been taken to bring them to life again. They are affected in the same manner, when they are killed in any other kind of noxious air that I have tried, viz. fixed air, inflammable air, air filled with the fumes of brimstone, infected with putrid matter, in which a mixture of iron filings and brimstone has stood, or in which charcoal has been burned, or metals calcined, or in nitrous air, &c.

If a mouse (which is an animal that I have commonly made use of for the purpose of these experiments) can stand the first shock of this stimulus, or has been habituated to it by degrees, it will live a considerable time in air in which other mice will die instantaneously. I have frequently found that when a number of mice have been confined in a given quantity of air, less than half the time that they have actually lived in it, a fresh mouse has been instantly thrown into convulsions, and died upon being put to them. It is evident, therefore, that if
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the experiment of the Black Hole were to be repeated, a man would stand the better chance of surviving it, who should enter at the first, than at the last hour. I have also observed, that young mice will always live much longer than old ones, or than those which are full grown, when they are confined in the same quantity of air. I have sometimes known a young mouse to live six hours in the same circumstances in which an old mouse has not lived one. On these accounts, experiments with mice, and, for the same reason, no doubt, with other animals also, have a considerable degree of uncertainty attending them; and therefore, it is necessary to repeat them frequently, before the result can be absolutely depended upon.

The discovery of the provision in nature for restoring air, which has been injured by the respiration of animals, having long appeared to me to be one of the most important problems in natural philosophy, I have tried a great variety of schemes in order to effect it. In these, my guide has generally been to consider the influences to which the atmosphere is, in fact, exposed; and, as some of my unsuccessful trials may be of use to those who are disposed to take pains in the farther investigation of this subject, I shall mention the principal of them.

The noxious effluvia with which air is loaded by animal respiration, is not absorbed by standing without agitation in fresh or salt water. I have kept it many months in fresh water, when, instead of being meliorated, it has seemed to become even more deadly, so as to require more time to restore it, by the methods which will be explained hereafter, than
air

air which has been lately made noxious. I have even spent several hours in pouring this air from one glass vessel into another, in water, sometimes as cold, and sometimes as warm, as my hands could bear it, and have sometimes also wiped the vessels many times, during the course of the experiment, in order to take off that part of the noxious matter, which might adhere to the glass vessels, and which evidently gave them an offensive smell; but all these methods were generally without any sensible effect. The motion, also, which the air received in these circumstances, it is very evident, was of no use for this purpose.

This kind of air is not restored by being exposed to the light, or by any other influence to which it is exposed, when confined in a thin phial, in the open air, for some months.

Among other experiments, I tried a great variety of different effluvia, which are continually exhaling into the air, especially of those substances which are known to resist putrefaction; but I could not by these means effect any melioration of the noxious quality of this kind of air.

Having read, in the Memoirs of the Imperial Society, of a plague not afflicting a particular village, in which there was a large sulphur work, I immediately fumigated a quantity of this kind of air; or (which will hereafter appear to be the very same thing) air tainted with putrefaction, with the fumes of burning brimstone, but without any effect.

I once imagined, that the nitrous acid in the air might be the general restorative which I was in quest of, and the conjecture was favoured, by find-
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ing that candles would burn, and animals live, in air extracted from saltpetre. I therefore spent a good deal of time in attempting, by a burning-glass, and other means, to impregnate this noxious air with some effluvia of saltpetre, and, with the same view, introduced into it the fumes of the smoaking spirit of nitre; but both these methods were altogether ineffectual.

In order to try the effect of heat, I put a quantity of air, in which mice had died, into a bladder, tied to the end of the stem of a tobacco-pipe, at the other end of which was another bladder, out of which the air was carefully pressed. I then put the middle part of the stem into a chafing-dish of hot coals, strongly urged with a pair of bellows; and, pressing the bladders alternately, I made the air pass several times through the heated part of the pipe. I have also made this kind of air very hot, standing in water before the fire. But neither of these methods were of any use.

Rarefaction and condensation by instruments were also tried, but in vain.

Thinking it possible that the earth might imbibe the noxious quality of the air, and thence supply the roots of plants with such putrescent matter as is known to be nutritive to them, I kept a quantity of air, in which mice had died, in a phial, one half of which was filled with fine garden mould; but, though it stood two months in these circumstances, it was not the better for it.

I once imagined that, since several kinds of air cannot be long separated from common air, by being confined in bladders, in bottles well corked, or even

closed with ground stopples, the affinity between this noxious air and the common air might be so great, that they would mix through a body of water interposed between them; the water continually receiving from the one, and giving to the other, especially as water receives some kinds of impregnation from, I believe, every kind of air to which it is contiguous; but I have seen no reason to conclude, that a mixture of any kind of air with the common air can be produced in this manner. I have kept air in which mice have died, air in which candles have burned out, and inflammable air, separated from the common air, by the slightest partition of water that I could well make, so that it might not evaporate in a day or two, if I should happen not to attend to them; but I found no change in them after a month or six weeks. The inflammable air was still inflammable, mice died instantly in the air in which other mice had died before, and candles would not burn where they had burned out before.

Since air tainted with animal or vegetable putrefaction is the same thing with air rendered noxious by animal respiration, I shall now recite the observations which I have made upon this kind of air, before I treat of the method of restoring them.

That these two kinds of air are, in fact, the same thing, I conclude from their having several remarkable common properties, and from their differing in nothing that I have been able to observe. They equally extinguish flame, they are equally noxious to animals, they are equally, and in the same way, offensive to the smell, they are equally diminished.

in their quantity, they equally precipitate in lime-water, and they are restored by the same means.

Since air which has passed through the lungs is the same thing with air tainted with animal putrefaction, it is probable that one use of the lungs is to carry off a putrid effluvium, without which, perhaps, a living body might putrefy as soon as a dead one.

When a mouse putrefies in any given quantity of air, the bulk of it is generally increased for a few days; but in a few days more it begins to shrink up, and generally, in about eight or ten days, if the weather be pretty warm, it will be found to be diminished $\frac{1}{3}$, or $\frac{2}{3}$ of its bulk. If it do not appear to be diminished after this time, it only requires to be passed through water, and the diminution will not fail to be sensible. I have sometimes known almost the whole diminution to take place, upon once or twice passing through the water. The same is the case with air, in which animals have breathed as long as they could. Also, air in which candles have burned out may almost always be farther reduced by this means. All these processes, as I observed before, seem to dispose the compound mass of air to part with some constituent part belonging to it; and this being miscible with water, must be brought into contact with it, in order to mix with it to the most advantage, especially when its union with the other constituent principles of the air is but partially broken.

I have put mice into vessels which had their mouths immersed in quicksilver, and observed that the air was not much contracted after they were dead or cold; but upon withdrawing the mice, and admitting

lime-water to the air it immediately became turbid, and was contracted in its dimensions as usual.

I tried the same thing with air tainted with putrefaction, putting a dead mouse to a quantity of common air, in a vessel which had its mouth immersed in quicksilver, and after a week I took the mouse out, drawing it through the quicksilver, and observed that for some time there was an apparent increase of the air perhaps about $\frac{1}{10}$. After this, it stood two days in the quicksilver, without any sensible alteration; and then admitting water to it, it began to be absorbed, and continued so, till the original quantity was diminished about $\frac{1}{2}$. If, instead of common water, I had made use of lime water in this experiment, I make no doubt but it would have become turbid.

If a quantity of lime-water in a phial be put under a glass vessel standing in water, it will not become turbid, and provided the access of the common air be prevented, it will continue lime-water, I do not know how long; but if a mouse be left to putrefy in the vessel, the water will deposit all its lime in a few days. This may be owing to the fixed air being transferred from the putrid mouse into the water, and yet it is evident that there is a putrid effluviu intirely distinct from this kind of air, and which has very different properties.

It is a doubt with me, however, whether the putrid effluviu be not chiefly fixed air, with the addition of some other effluviu, which has the power of diminishing common air. The resemblance between the true putrid effluviu and fixed air in the following experiment, which is as decisive

as I can possibly contrive it, appeared to be very great; indeed, much greater than I had expected. I put a dead mouse into a tall glass vessel, and having filled the remainder with quicksilver, and set it, inverted, in a pot of quicksilver, I let it stand about two months, in which time the putrid effluvium issuing from the mouse had filled the whole vessel, and part of the dissolved blood, which lodged upon the surface of the quicksilver, began to be thrown out. I then filled another glass vessel, of the same size and shape, with as pure fixed air as I could make, and exposed them both, at the same time, to a quantity of lime-water. In both cases the water grew turbid alike, it rose equally fast in both the vessels, and likewise equally high; so that about the same quantity remained unabsorbed by the water. One of these kinds of air, however, was exceedingly sweet and pleasant, and the other insufferably offensive; one of them also would have made an addition to any quantity of common air with which it had been mixed, and the other would have diminished it. This, at least, would have been the consequence, if the mouse itself had putrefied in any quantity of air.

It seems to depend, in some measure, upon the time, and other circumstances, in the dissolution of animal or vegetable substances, whether they yield the proper putrid effluvium, or fixed, or inflammable air; but the experiments which I have made upon this subject, have not been numerous enough to enable me to decide with certainty concerning those circumstances. Putrid cabbage, green, or boiled, infects the air in the very same manner as putrid animal substances. Air thus tainted is equally contracted
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in its dimensions, it equally extinguishes flame, and is equally noxious to animals; but they affect the air very differently if the heat that is applied to them be considerable. If beef or mutton, raw, or boiled, be placed so near to the fire, that the heat to which it is exposed shall equal, or rather exceed, that of the blood, a considerable quantity of air will be generated in a day or two, about $\frac{1}{3}$ th of which I have generally found to be absorbed by water, while all the rest was inflammable; but air generated from vegetables, in the same circumstances, will be almost all fixed, and no part of it inflammable. This I have repeated again and again, the whole process being in quicksilver; so that neither common air, nor water, had any access to the substance on which the experiment was made; and the generation of air, or effluvium of any kind, except what might be absorbed by quicksilver, or reformed by the substance itself, might be distinctly noted.

A vegetable substance, after standing a day or two in these circumstances, will yield nearly all the air that can be extracted from it, in that degree of heat; whereas an animal substance will continue to give more air or effluvium, of some kind or other, with very little alteration, for many weeks. It is remarkable, however, that though a piece of beef or mutton, plunged in quicksilver, and kept in this degree of heat, yield air, the bulk of which is inflammable, and contracts no putrid smell (at least, in a day or two), a mouse treated in the same manner, yields the proper putrid effluvium, as, indeed the smell sufficiently indicates; and this effluvium does
either

either itself extinguish flame, or has in it such a mixture of fixed air, as to give it that property.

That the putrid effluviū will mix with water seems to be evident from the following experiment. If a mouse be put into a jar full of water, standing with its mouth inverted in another vessel of water, a considerable quantity of elastic matter (and which may, therefore, be called air) will soon be generated, unless the weather be so cold as to check all putrefaction. After a short time, the water contracts an extremely fetid and offensive smell, which seems to indicate that the putrid effluviū pervades the water, and affects the neighbouring air; and since, after this, there is often no increase of the air, that seems to be the very substance which is carried off through the water, as fast as it is generated; and the offensive smell is a sufficient proof that it is not fixed air. For this has a very agreeable flavour, whether it be produced by fermentation, or extracted from chalk by oil of vitriol; affecting not only the mouth, but even the nostrils, with a pungency which is peculiarly pleasing to a certain degree, as any person may easily satisfy himself who will chuse to make the experiment. If the water in which the mouse was immersed, and which is saturated with the putrid air, be changed, the greater part of the putrid air will, in a day or two, be absorbed, though the mouse continues to yield the putrid effluviū as before; for as soon as this fresh water becomes saturated with it, it begins to be offensive to the smell, and the quantity of the putrid air upon its surface increases as before. I kept a mouse producing putrid air in this manner for the space of several months.

Six ounce measures of air not readily absorbed by water, appeared to have been generated from one mouse, which had been putrefying eleven days in confined air, before it was put into a jar which was quite filled with water, for the purpose of this observation.

Air thus generated from putrid mice standing in water, without any mixture of common air, extinguishes flame, and is noxious to animals, but not more so than common air only tainted with putrefaction. It is exceedingly difficult and tedious to collect a quantity of this putrid air, not miscible in water, so very great a proportion of what is collected being absorbed by the water, in which it is kept; but what that proportion is, I have not endeavoured to ascertain.

Though a quantity of air be diminished by any substance putrefying in it, I have not yet found the same effect to be produced by a mixture of putrid air with common air; but, in the manner in which I have hitherto made the experiment, I was obliged to let the putrid air, pass through a body of water; which might instantly absorb whatever it was in the putrid substance, that diminished the common air.

Insects of various kinds live perfectly well in air tainted with animal or vegetable putrefaction, when a single inspiration of it would have instantly killed any animal. I have frequently tried the experiment with flies and butterflies. I have also observed, that the *aphides* will thrive as well upon plants growing in this kind of air, as in the open air. I have even been frequently obliged to take plants out of the putrid air in which they were growing, on purpose to brush away the swarms of
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these insects which infected them; and yet so effectually did some of them conceal themselves, and so fast did they multiply, in these circumstances, that I could seldom keep the plants quite clear of them.

When air has been freshly and strongly tainted with putrefaction, so as to smell through the water, sprigs of mint have presently died, upon being put into it, their leaves turning black; but if they do not die presently, they thrive in a most surprising manner. In no other circumstances have I ever seen vegetation so vigorous as in this kind of air, which is immediately fatal to animal life. Though these plants have been crowded in jars filled with this air, every leaf has been full of life; fresh shoots have branched out in various directions, and have grown much faster than other similar plants, growing in the same exposure in common air.

This observation led me to conclude, that plants, instead of affecting the air in the same manner with animal respiration, reverse the effects of breathing, and tend to keep the atmosphere sweet and wholesome, when it is become noxious, in consequence of animals living and breathing, or dying and putrefying in it.

In order to ascertain this, I took a quantity of air, made thoroughly noxious, by mice breathing and dying in it, and divided it into two parts; one of which I put into a phial immersed in water; and to the other (which was contained in a glass jar, standing in water) I put a sprig of mint. This was about the beginning of August 1771, and after eight or nine days, I found that a mouse lived perfectly well

in that part of the air, in which the sprig of mint had grown, but died the moment it was put into the other part of the same original quantity of air; and which I had kept in the very same exposure, but without any plant growing in it.

This experiment I have several times repeated; sometimes using air, in which animals had breathed and died; sometimes using air tainted with vegetable or animal putrefaction, and generally with the same success.

Once, I let a mouse live and die in a quantity of air, which had been noxious, but which had been restored by this process, and it lived nearly as long as I conjectured it might have done in an equal quantity of fresh air; but, this is so exceedingly various, that it is not easy to form any judgment from it; and in this case the symptom of *difficult respiration* seemed to begin earlier than it would have done in common air.

Since the plants that I made use of manifestly grow and thrive in putrid air; since putrid matter is well known to afford proper nourishment for the roots of plants; and since it is likewise certain that they receive nourishment by their leaves as well as by their roots, it seems to be exceedingly probable, that the putrid effluviū is in some measure extracted from the air, by means of the leaves of plants, and therefore that they render the remainder more fit for respiration.

Towards the end of the year some experiments of this kind did not answer so well as they had done before, and I had instances of the relapsing of this restored air to its former noxious state. I therefore suspended

suspended my judgment concerning the efficacy of plants to restore this kind of noxious air, till I should have an opportunity of repeating my experiments, and giving more attention to them. Accordingly I resumed the experiments in the summer of the year 1772, when I presently had the most indisputable proof of the restoration of putrid air by vegetation; and as the fact is of some importance, and the subsequent variation in the state of this kind of air is a little remarkable; I think it necessary to relate some of the facts pretty circumstantially.

The air, on which I made the first experiments, was rendered exceedingly noxious by mice dying in it on the 20th of June. Into a jar nearly filled with one part of this air, I put a sprig of mint, while I kept another part of it in a phial, in the same exposure; and on the 27th of the same month, and not before, I made a trial of it, by introducing a mouse into a glass vessel, containing $2\frac{1}{2}$ ounce measures filled with each kind of air; and I noted the following facts.

When the vessel was filled with the air in which the mint had grown, a very large mouse lived five minutes in it, before it began to shew any sign of uneasiness. I then took it out, and found it to be as strong and vigorous as when it was first put in; whereas in that air which had been kept in the phial only, without a plant growing in it, a younger mouse continued not longer than two or three seconds, and was taken out quite dead. It never breathed after, and was immediately motionless. After half an hour, in which time the larger mouse

(which I had kept alive, that the experiment might be made on both the kinds of air with the very same animal) would have been sufficiently recruited, supposing it to have received any injury by the former experiment, was put into the same vessel of air ; but though it was withdrawn again, after being in it hardly one second, it was recovered with difficulty, not being able to stir from the place for near a minute. After two days, I put the same mouse into an equal quantity of common air, and observed that it continued seven minutes without any sign of uneasiness; and being very uneasy after three minutes longer, I took it out. Upon the whole, I concluded that the restored air wanted about one fourth of being as wholesome as common air. The same thing also appeared when I applied the test of nitrous air.

In the seven days, in which the mint was growing in this jar of noxious air, three old shoots had extended themselves about three inches, and several new ones had made their appearance in the same time. Dr. Franklin and Sir John Pringle happened to be with me, when the plant had been three or four days in this state, and took notice of its vigorous vegetation, and remarkably healthy appearance in that confinement.

On the 30th of the same month, a mouse lived fourteen minutes, breathing naturally all the time, and without appearing to be much uneasy, till the last two minutes, in air which had been rendered noxious by mice breathing in it almost a year before, and which I had found to be most highly noxious on the 10th of this month, a plant having grown in it.

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but not exceedingly well, these eleven days; on which account, I had deferred making the trial so long. This restored air was affected by a mixture of nitrous air, almost as much as common air.

As this putrid air was thus easily restored to a considerable degree of fitness for respiration, by plants growing in it, I was in hopes that by the same means it might in time be so much more perfectly restored, that a candle would burn in it; and for this purpose I kept plants growing in the jars which contained this air till the middle of August following, but did not take sufficient care to pull out all the old and rotten leaves. The plants, however, had grown, and looked so well upon the whole, that I had no doubt but that the air must constantly have been in a mending state; when I was exceedingly surprized to find, on the 24th of that month, that though the air in one of the jars had not grown worse, it was no better, and that the air in the other jar was so much worse than it had been, that a mouse would have died in it in a few seconds. It also made no effervescence with nitrous air, as it had done before.

Suspecting that the same plant might be capable of restoring putrid air to a certain degree only, or that plants might have a contrary tendency in some stages of their growth, I withdrew the old plant, and put a fresh one in its place; and found that, after seven days, the air was restored to its former wholesome state. This fact I consider as a very remarkable one, and well deserving of a farther investigation, as it may throw more light upon the principles of vegetation. It is not, however,

a single fact ; for I had several instances of the same kind in the preceding year ; but it seemed so very extraordinary, that air should grow worse by the continuance of the same treatment by which it had grown better, that, whenever I observed it, I concluded that I had not taken sufficient care to satisfy myself of its previous restoration.

That plants are capable of perfectly restoring air injured by respiration, may, I think, be inferred with certainty from the perfect restoration, by this means, of air which had passed through my lungs, so that a candle would burn in it again, though it had extinguished flame before, and a part of the same original quantity of air still continued to do so. Of this one instance occurred in the year 1771, a sprig of mint having grown in a jar of this kind of air, from the 25th of July to the 17th of August following ; and another trial I made with the same success the 7th of July 1772, the plant having grown in it from the 29th of June preceding. In this case also I found that the effect was not owing to any virtue in the leaves of mint ; for I kept them constantly changed in a quantity of this kind of air, for a considerable time, without making any sensible alteration in it.

These proofs of a partial restoration of air by plants in a state of vegetation, though in a confined and unnatural situation, cannot but render it highly probable, that the injury which is continually done to the atmosphere by the respiration of such a number of animals, and the putrefaction of such masses of both vegetable and animal matter, is, in part at least, repaired by the vegetable creation.

And,

And, notwithstanding the prodigious mass of air that is corrupted daily by the abovementioned causes; yet, if we consider the immense profusion of vegetables upon the face of the earth, growing in places suited to their nature, and consequently at full liberty to exert all their powers, both inhaling and exhaling, it can hardly be thought, but that it may be a sufficient counterbalance to it, and that the remedy is adequate to the evil.

Dr. Franklin, who, as I have already observed, saw some of my plants in a very flourishing state, in highly noxious air, was pleased to express very great satisfaction with the result of the experiments. In his answer to the letter in which I informed him of it, he says,

“ That the vegetable creation should restore the
 “ air which is spoiled by the animal part of it,
 “ looks like a rational system, and seems to be of
 “ a piece with the rest. Thus fire purifies water
 “ all the world over. It purifies it by distillation,
 “ when it raises it in vapours, and lets it fall in
 “ rain; and farther still by filtration, when, keep-
 “ ing it fluid, it suffers that rain to percolate the
 “ earth. We knew before, that putrid animal sub-
 “ stances were converted into sweet vegetables,
 “ when mixed with the earth, and applied as
 “ manure; and now, it seems, that the same pu-
 “ trid substances, mixed with the air, have a simi-
 “ lar effect. The strong thriving state of your
 “ mint in putrid air seems to shew that the air is
 “ mended by taking something from it, and not
 “ by adding to it.” He adds, “ I hope this will
 “ give some check to the rage of destroying trees.
 “ that

“ that grow near houses, which has accompanied
 “ our late improvements in gardening, from an
 “ opinion of their being unwholesome. I am cer-
 “ tain, from long observation, that there is no-
 “ thing unhealthy in the air of woods; for we
 “ Americans have every where our country habi-
 “ tations in the midst of woods, and no people on
 “ earth enjoy better health, or are more prolific.”

Having rendered inflammable air perfectly in-
 noxious by continued agitation in a trough of water,
 deprived of its air, I concluded that other kinds of
 noxious air might be restored by the same means;
 and I presently found that this was the case with
 putrid air, even of more than a year's standing. I
 shall observe once for all, that this process has ne-
 ver failed to restore any kind of noxious air on
 which I have tried it, viz. air injured by respira-
 tion or putrefaction, air infected with the fumes
 of burning charcoal, and of calcined metals, air
 in which a mixture of iron filings and brimstone,
 or that in which paint made of white lead and oil
 has stood, or air which has been diminished by a
 mixture of nitrous air. Of the remarkable effect
 which this process has on nitrous air itself, an ac-
 count will be given in its proper place.

If this process be made in water deprived of air,
 either by the air pump, by boiling, by distillation,
 or if fresh rain water be used, the air will always
 be diminished by the agitation; and this is cer-
 tainly the fairest method of making the experi-
 ment. If the water be fresh pump water, there
 will always be an increase of the air by agitation,
 the air contained in the water being set loose, and
 joining

joining that which is in the jar. In this case, also, the air has never failed to be restored; but then it might be suspected that the melioration was produced by the addition of some more wholesome ingredient. As these agitations were made in jars with wide mouths, and in a trough which had a large surface exposed to the common air, I take it for granted that the noxious effluvia, whatever they be, were first imbibed by the water, and thereby transmitted to the common atmosphere. In some cases this was sufficiently indicated by the disagreeable smell which attended the operation.

After I had made these experiments, I was informed that an ingenious physician and philosopher had kept a fowl alive twenty-four hour, in a quantity of air in which another fowl of the same size had not been able to live longer than an hour, by contriving to make the air, which it breathed, pass through no very large quantity of acidulated water, the surface of which was not exposed to the common air; and that even when the water was not acidulated, the fowl lived much longer than it could have done, if the air which it breathed had not been drawn through the water. As I should not have concluded that this experiment would have succeeded so well, from any observations that I had made upon the subject, I took a quantity of air in which mice had died, and agitated it very strongly, first in about five times its own quantity of distilled water, in the manner in which I had impregnated water with fixed air; but though the operation was continued a long time, it made no sensible change in the properties of the air. I also repeated the operation with

pump water, but with as little effect. In this case, however, though the air was agitated in a phial, which had a narrow neck, the surface of the water in the basin was considerably large, and exposed to the common atmosphere, which must have tended a little to favour the experiment. In order to judge more precisely of the effect of these different methods of agitating air, I transferred the very noxious air, which I had not been able to amend in the least degree by the former method, into an open jar, standing in a trough of water; and when I had agitated it till it was diminished about one third, I found it to be better than air, in which candles had burned out, as appeared by the test of the nitrous air; and a mouse lived in $2\frac{1}{4}$ ounce measures of it a quarter of an hour, and was not sensibly affected the first ten or twelve minutes.

In order to determine whether the addition of any *acid* to the water, would make it more capable of restoring putrid air, I agitated a quantity of it in a phial containing very strong vinegar; and after that in *aqua fortis*, only half diluted with water; but, by neither of these processes was the air at all mended, though the agitation was repeated at intervals during a whole day, and it was moreover allowed to stand in that situation all night.

Since, however, water in these experiments must have imbibed and retained a certain portion of the noxious effluvia, before they could be transmitted to the external air, I do not think it improbable but that the agitation of the sea and large lakes may be of some use for the purification of the atmosphere, and the putrid matter contained in water may be imbibed

imbibed by aquatic plants, or be deposited in some other manner.

Having found, by several experiments above-mentioned, that the proper putrid effluvium is something quite distinct from fixed air, and finding, by the experiments of Dr. Macbride, that fixed air corrects putrefaction; I once concluded that this effect was produced, not by stopping the flight of the fixed air, or restoring to the putrefying substance the very same thing that had escaped from it; and which was the common vinculum of all its parts (which is that ingenious author's hypothesis) but by an affinity between the fixed air and the putrid effluvium. It therefore occurred to me, that fixed air, and air tainted with putrefaction, though equally noxious when separate, might make a wholesome mixture, the one correcting the other; and I was confirmed in this opinion by, I believe, not less than fifty or sixty instances, in which air, that had been made in the highest degree noxious, by respiration or putrefaction, was so far sweetened, by a mixture of about four times as much fixed air that afterwards mice lived in it exceedingly well, and in some cases almost as long as in common air. I found it, indeed, to be more difficult to restore old putrid air by this means; but I hardly ever failed to do it, when the two kinds of air had stood a long time together, by which I mean about a fortnight or three weeks.

The reason why I do not absolutely conclude that the restoration of air in these cases was the effect of fixed air, is that, when I made a trial of the mixture, I sometimes agitated the two kinds

of air pretty strongly together, in a trough of water, or at least passed it several times through the water, from one jar to another, that the superfluous fixed air might be absorbed, not suspecting at that time that the agitation could have any other effect; but having since found that very violent, and especially long continued agitation in water, without any mixture of fixed air, never failed to render any kind of noxious air in some measure fit for respiration (and in one particular instance the mere transferring of the air from one vessel to another through the water, though for a much longer time than I ever used for the mixtures of air, was of considerable use for the same purpose); I began to entertain some doubt of the efficacy of fixed air, for that purpose. In some cases also the mixture of fixed air had by no means so much effect on the putrid air as, from the generality of my observations, I should have expected.

I was always aware, indeed, that it might be said, that the residuum of fixed air not being very noxious, such an addition must contribute to mend the putrid air; but, in order to obviate this objection, I once mixed the residuum of as much fixed air as I had found, by a variety of trials, to be sufficient to restore a given quantity of putrid air, with an equal quantity of putrid air, without making any sensible melioration of it.

Upon the whole, I am inclined to think that this process could hardly have succeeded so well as it did with me, and in so great a number of trials, unless fixed air have some tendency to correct air tainted with respiration or putrefaction; and it is perfectly

perfectly agreeable to the analogy of Dr. Macbride's discoveries, and may naturally be expected from them, that it should have such an effect.

By a mixture of fixed air I have made wholesome the residuum of air generated by putrefaction only, from mice plunged in water. This, one would imagine, *à priori*, to be the most noxious of all kinds of air. For if common air only tainted with putrefaction be so deadly, much more might one expect that air to be so, which was generated from putrefaction only; but it seems to be nothing more than common air tainted with putrefaction, and therefore requires no other process to sweeten it. In this case, however, we seem to have an instance of the generation of genuine common air, though mixed with something that is foreign to it. Perhaps the residuum of fixed air may be another instance of the same nature.

Fixed air is equally diffused through the whole mass of any quantity of putrid air with which it is mixed; for dividing the mixture into two equal parts, they were reduced in the same proportion by passing through water. But this is also the case with some of the kinds of air which will not incorporate, as inflammable air, and air in which brimstone has burned.

If fixed air tend to correct air which has been injured by animal respiration or putrefaction, limekilns; which discharge great quantities of fixed air, may be wholesome in the neighbourhood of populous cities, the atmosphere of which must abound with putrid effluvia. I should think also that physicians might avail themselves of the application
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of fixed air in many putrid disorders, especially as it may be so easily administered by way of clyster, where it would often find its way to much of the putrid matter. Nothing is to be apprehended from the distention of the bowels by this kind of air, since it is so readily absorbed by any fluid or moist substance. Since fixed air is not noxious *per se*, but, like fire, only in excess, I do not think it at all hazardous to attempt to breathe it. It is however easily conveyed into the stomach, in natural or artificial Pyrmont water, in briskly fermenting liquors, or a vegetable diet. It is possible, however, that a considerable quantity of fixed air might be imbibed by the absorbing vessels of the skin, if the whole body, except the head, should be suspended over a vessel of strongly fermenting liquor; and in some putrid disorders this treatment might be very salutary. If the body was exposed quite naked, there would be very little danger from the cold in this situation, and the air having freer access to the skin might produce a greater effect. Being no physician, I run no risk by throwing out these random, and perhaps whimsical, proposals.

Having communicated my observations on fixed air, and especially my scheme of applying it by way of *clyster* in putrid disorders, to Mr. Hey, an ingenious surgeon in this town, a case presently occurred, in which he had an opportunity of giving it a trial; and mentioning it to Dr. Hird and Dr. Crowther, two physicians who attended the patient, they approved the scheme, and it was put in execution: both by applying the fixed air by way of clyster, and at the same time making the

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patient

patient drink plentifully of liquors strongly impregnated with it. The event was such, that I requested Mr. Hey to draw up a particular account of the case, describing the whole of the treatment, that the public might be satisfied that this new application of fixed air is perfectly safe, and also have an opportunity of judging how far it had the effect which I expected from it; and as the application is new, and not unpromising, I shall beg leave to subjoin his letter to me on the subject, by way of *Appendix* to these papers.

V.

OF AIR IN WHICH A MIXTURE OF BRIMSTONE.
AND FILINGS OF IRON HAS STOOD.

Finding in Dr. Hales's account of his experiments, that there was a great diminution of the quantity of air in which a mixture of powdered brimstone and filings of iron, made into a paste with water, had stood, I repeated the experiment, and found the diminution greater than I had expected. The diminution of air by this process is made as effectually, and as expeditiously, in quicksilver as in water; and it may be measured with the greatest accuracy, because there is neither any previous expansion nor increase of the quantity of air, and because it is some time before it begins to have any sensible effect. The diminution of air by this process is various; but I have generally

generally found it to be between $\frac{1}{2}$ and $\frac{1}{3}$ of the whole.

Air thus diminished is not heavier, but rather lighter than common air; and though lime-water does not become turbid when it is exposed to this air, it is probably owing to the formation of a selenitic salt, as was the case with the simple burning of brimstone abovementioned. That something proceeding from the brimstone strongly affects the water which is confined in the same place with this brimstone, is manifest from the very strong smell that it has of the volatile spirit of vitriol. I conclude the diminution of air by this process is of the same kind with the diminution of it in the other cases, because when this mixture is put into air which has been previously diminished, either by the burning of candles, by respiration, or putrefaction, though it never fails to diminish it something more, it is, however, no farther than this process alone would have done it. If a fresh mixture be introduced into a quantity of air which had been reduced by a former mixture, it has little or no farther effect.

I observed, that when a mixture of this kind was taken out of a quantity of air in which a candle had before burned out, and in which it had stood for several days, it was quite cold and black, as it always becomes in a confined place; but it presently grew very hot, smoked copiously, and smelled very offensively; and when it was cold, it was brown, like the rust of iron.

I once put a mixture of this kind to a quantity of inflammable air, made from iron, by which means it was diminished $\frac{1}{2}$ or $\frac{1}{3}$ in its bulk; but, as far as I could

I could judge, it was still as inflammable as ever. Another quantity of inflammable air was also reduced in the same proportion, by a mouse putrefying in it; but its inflammability was not seemingly lessened.

Air diminished by this mixture of iron filings and brimstone, is exceedingly noxious to animals, and I have not perceived that it grows any better by keeping in water. The smell of it is very pungent and offensive.

The quantity of this mixture which I made use of in the preceding experiments, was from two to four ounce measures; but I did not perceive, but that the diminution of the quantity of air (which was generally about twenty ounce measures) was as great with the smallest, as with the largest quantity. How small a quantity is necessary to diminish a given quantity of air to a *maximum*, I have made no experiments to ascertain.

As soon as this mixture of iron filings, with brimstone and water, begins to ferment, it also turns black, and begins to swell, and it continues to do so, till it occupies twice as much space as it did at first; and the force with which it expands is great; but how great it is I have not endeavoured to determine.

When this mixture is immersed in water, it generates no air, though it becomes black, and swells.

VI.

OF NITROUS AIR.

Ever since I first read Dr. Hales's most excellent Statical Essays, I was particularly struck with that experiment of his, of which an account is given, Vol. I. p. 224, and Vol. II. p. 280: in which common air, and air generated from the Walton pyrites, by spirit of nitre, made a turbid red mixture, and in which part of the common air was absorbed; but I never expected to have the satisfaction of seeing this remarkable appearance, supposing it to be peculiar to that particular mineral. Happening to mention this subject to the Hon. Mr. Cavendish, when I was in London, in the spring of the year 1772, he said that he did not imagine but that other kinds of pyrites might answer as well as that which Dr. Hales made use of, and that probably the red appearance of the mixture depended upon the spirit of nitre only. This encouraged me to attend to the subject; and having no pyrites, I began with the solution of the different metals in spirit of nitre, and catching the air which was generated in the solution, I presently found what I wanted, and a good deal more.

Beginning with the solution of brass, on the 4th of June 1772, I first found this remarkable species of air; one effect of which, though it was casually observed by Dr. Hales, he gave but little attention to; and which, as far as I know, has passed altogether unnoticed since his time, insomuch that no name has been given to it. I therefore found myself, contrary

to my first resolution, under an absolute necessity of giving a name to this kind of air myself. When I first began to speak and write of it to my friends, I happened to distinguish it by the name of nitrous air, because I had procured it by means of spirit of nitre only; and though I cannot say that I altogether like the term, because this air is not got from all the metals by the same spirit, neither myself nor any of my friends, to whom I have applied for the purpose, have been able to hit upon a better; so that I am obliged, after all, to content myself with it.

I have found that this kind of air is readily procured from iron, copper, brass, tin, silver, quicksilver, bismuth, and nickel, by the nitrous acid only, and from gold and the regulus of antimony by aqua regia. The circumstances attending the solution of each of these metals are various, but hardly worth mentioning, in treating of the properties of the air which they yield, which, from what metal soever it is extracted, has, as far as I have been able to observe, the very same properties.

One of the most conspicuous properties of this kind of air is the great diminution of any quantity of common air with which it is mixed, attended with a turbid red, or deep orange colour, and a considerable heat. The smell of it, also, is very strong, and remarkable, but very much resembling that of smoking spirit of nitre.

The diminution of a mixture of this and common air is not an equal diminution of both the kinds, which is all that Dr. Hales could observe, but of the common air chiefly, though not wholly. For if one measure of nitrous air be put to two measures of
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common

common air, in a few minutes (by which time the effervescence will be over, and the mixture will have recovered its transparency) there will want about one ninth of the original two measures. I hardly know any experiment that is more adapted to amaze and surprize than this is, which exhibits a quantity of air, which, as it were, devours a quantity of another kind of air half as large as itself, and yet is so far from gaining any addition to its bulk, that it is diminished by it. If, after this full saturation of common air with nitrous air, more nitrous air be put to it, it makes an addition equal to its own bulk, without producing the least redness, or any other visible effect.

That this diminution is chiefly in the quantity of common air, is evident from this observation, that if the smallest quantity of common air be put to any larger quantity of nitrous air, though the two together will not occupy so much space as they did separately, yet the quantity will be still larger than that of the nitrous air only. One ounce measure of common air being put to near twenty ounce measures of nitrous air, made an addition to it of about half an ounce measure. This, however, being a much greater proportion than the diminution of common air, in the former experiment, seems to prove that part of the diminution in the former case is in the nitrous air. Besides, it will presently appear, that nitrous air is subject to a most remarkable diminution; and as common air, in a variety of other cases, suffers a diminution from one fifth to one fourth, I conclude, that in this case also it does not exceed that proportion, and therefore that the remainder of the diminution respects the nitrous air.

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In order to judge whether the water contributed to the diminution of this mixture of nitrous and common air, I made the whole process several times in quicksilver, using one third of nitrous, and two thirds of common air, as before. In this case the redness continued a very long time, and the diminution was not so great as when the mixtures had been made in water, there remaining one seventh more than the original quantity of common air. This mixture stood all night upon the quicksilver; and the next morning I observed that it was no farther diminished upon the admission of water to it, nor by pouring it several times through the water, and letting it stand in water two days. Another mixture, which stood about six hours on the quicksilver, was diminished a little more upon the admission of water, but was never less than the original quantity of common air. In another case, however, in which the mixture stood but a very short time in quicksilver, the farther diminution, which took place upon the admission of water, was much more considerable; so that the diminution, upon the whole, was very nearly as great as if the process had been entirely in water. It is evident from these experiments, that the diminution is in part owing to the absorption by the water; but that when the mixture is kept a long time, in a situation in which there is no water to absorb any part of it, it acquires a constitution, by which it is afterwards incapable of being absorbed by water.

In order to determine whether the fixed part of common air was deposited in the diminution of it
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by nitrous air, I inclosed a vessel full of lime water in the jar in which the process was made, but it occasioned no precipitation of the lime; and when the vessel was taken out, after it had been in that situation a whole day, the lime was easily precipitated by breathing into it as usual.

It is exceedingly remarkable that this effervescence and diminution, occasioned by the mixture of nitrous air, is peculiar to common air, or air fit for respiration; and, as far as I can judge, from a great number of observations, is at least very nearly, if not exactly, in proportion to its fitness for this purpose; so that by this means the goodness of air may be distinguished much more accurately than it can be done by putting mice, or any other animals, to breathe in it. This was a most agreeable discovery to me, as I hope it may be an useful one to the public; especially as, from this time, I had no occasion for so large a stock of mice as I had been used to keep for the purpose of these experiments, using them only in those which required to be very decisive; and in these cases I have seldom failed to know beforehand in what manner they would be affected.

It is also remarkable that, on whatever account air is unfit for respiration, this same test is equally applicable. Thus there is not the least effervescence between nitrous and fixed air, or inflammable air, or any species of diminished air. Also the degree of diminution being from nothing at all to more than one third of the whole of any quantity of air, we are by this means in possession of a prodigiously large scale, by which we may distinguish
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very small degrees of difference in the goodness of air. I have not attended much to this circumstance, having used this test chiefly for greater differences; but, if I did not deceive myself, I have perceived a real difference in the air of my study, after a few persons have been with me in it; and the air on the outside of the house. Also a phial of air having been sent me, from the neighbourhood of York, it appeared not to be so good as the air near Leeds; that is, it was not diminished so much by an equal mixture of nitrous air, every other circumstance being as nearly the same as I could contrive. It may perhaps be possible, but I have not yet attempted it, to distinguish some of the different winds, or the air of different times of the year, by this test.

By means of this test I was able to determine what I was before in doubt about, *viz.* the kind as well as the degree of injury done to air by candles burning in it. I could not tell with certainty by means of mice, whether it was at all injured with respect to respiration; and yet if nitrous air may be depended upon for furnishing an accurate test, it must be rather more than one third worse than common air, and have been diminished by the same general cause of the other diminutions of air. For when, after many trials, I put one measure of thoroughly putrid and highly noxious air, into the same vessel with two measures of good wholesome air, and into another vessel an equal quantity, *viz.* three measures of air in which a candle had burned out; and then put equal quantities of nitrous air to each of them, the former was diminished rather more than the latter. It agrees
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with this observation, that burned air is farther diminished both by putrefaction, and a mixture of iron filings and brimstone; and I therefore, take it for granted, by every other cause of the diminution of air. It is probable, therefore, that burned air is air so far loaded with phlogiston, as to be able to extinguish a candle, which it may do long before it is fully saturated.

Inflammable air with a mixture of nitrous air burns with a green flame. This makes a very pleasing experiment when it is properly conducted. As, for some time, I chiefly made use of copper for the generation of nitrous air, I first ascribed this circumstance to that property of this metal, by which it burns with a green flame; but I was presently satisfied that it must arise from the spirit of nitre, for the effect is the very same from whichever of the metals the nitrous air is extracted, all of which I tried for this purpose, even silver and gold. A mixture of oil of vitriol and spirit of nitre in equal proportions dissolved iron, and the produce was nitrous air; but a less degree of spirit of nitre in the mixture produced air that was inflammable, and which burned with a green flame. It also tinged common air a little red, and diminished it, though not much.

The diminution of common air by a mixture of nitrous air, is not so extraordinary as the diminution which nitrous air itself is subject to from a mixture of iron filings and brimstone, made into a paste with water. This mixture, as I have already observed, diminishes common air between one fifth and one fourth, but has no such effect upon
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any kind of air that has been diminished, and rendered noxious by any other process; but when it is put to a quantity of nitrous air, it diminishes it so much, that no more than one fourth of the original quantity will be left. The effect of this process is generally perceived in five or six hours, about which time the visible effervescence of the mixture begins; and in a very short time it advances so rapidly, that in about an hour almost the whole effect will have taken place. If it be suffered to stand a day or two longer, the air will still be diminished farther, but only a very little farther, in proportion to the first diminution. The glass jar, in which the air and this mixture have been confined, has generally been so much heated in this process, that I have not been able to touch it.

Nitrous air thus diminished has not the peculiar smell of nitrous air, but smells just like common air in which the same mixture has stood; and it is not capable of being diminished any farther, by a fresh mixture of iron and brimstone.

Common air saturated with nitrous air is also no farther diminished by this mixture of iron filings and brimstone, though the mixture ferments with great heat, and swells very much in it.

Plants die very soon, both in nitrous air, and also in common air saturated with nitrous air, but especially in the former.

Neither nitrous air, nor common air saturated with nitrous air, differs in specific gravity from common air, or, at least, so little, that I could

not be sure of it, sometimes about three pints of it seeming to be about half a grain heavier, and at other times as much lighter than common air.

Having, among other kinds of air, exposed a quantity of nitrous air, to water out of which the air had been well boiled, in the experiment to which I have more than once referred, as having been the occasion of several new and important observations, I found that $\frac{1}{2}$ of the whole was absorbed. Perceiving, to my great surprize, that so very great a proportion of this kind of air was miscible with water, I immediately began to agitate a considerable quantity of it, in a jar standing in a trough of the same kind of water; and with about four times as much agitation as fixed air requires, it was so far absorbed by the water, that only about one fifth remained. This remainder extinguished flame, and was noxious to animals. Afterwards I diminished a pretty large quantity of it to one eighth of its original bulk, and the remainder still retained much of its peculiar smell, and diminished common air a little. A mouse also died in it, but not so suddenly as it would have done in pure nitrous air. In this operation the peculiar smell of nitrous air is very manifest, the water being first impregnated with the air, and then transmitting it to the common atmosphere.

This experiment gave me the hint of impregnating water with nitrous air, in the manner in which I had before done it with fixed air; and I presently found that distilled water would imbibe about one tenth of its bulk of this kind of air, and that

that it acquired a remarkably acid and astringent taste from it. The smell of water thus impregnated is at first peculiarly pungent. I did not chuse to swallow any of it, though, for any thing that I know, it may be perfectly innocent, and perhaps, in some cases, salutary.

This kind of air is retained very obstinately by water. In an exhausted receiver a quantity of water thus saturated emitted a whitish fume, such as sometimes issues from bubbles of this air when it is first generated, and also some air bubbles; but though it was suffered to stand a long time in this situation, it still retained its peculiar taste; but when it had stood all night pretty near the fire, the water was become quite vapid, and had deposited a filmy kind of matter, of which I had often collected a considerable quantity from the trough in which jars containing this air had stood. This I suppose to be a precipitate of the metal by the solution of which the nitrous air was generated. I have not given so much attention to it as to know, with certainty, in what circumstances this deposit is made, any more than I do the matter deposited from inflammable air abovementioned; for I cannot get it, at least in any considerable quantity, when I please; whereas I have often found abundance of it, when I did not expect it at all.

The nitrous air with which I made the first impregnation of water was extracted from copper; but when I made the impregnation with air from quicksilver, the water had the very same taste, though the matter deposited from it seemed to be of a dif-

ferent kind ; for it was whitish, whereas the other had a yellowish tinge. Except the first quantity of this impregnated water, I could never deprive any more that I made of its peculiar taste. I have even let some of it stand more than a week, in phials with their mouths open, and sometimes very near the fire, without producing any alteration in it.

Whether any of the spirit of nitre be properly contained in the nitrous air, and be mixed with the water in this operation, I have not yet endeavoured to determine. This, however, may probably be the case, as the spirit of nitre is in a considerable degree volatile.

It will perhaps be thought, that the most useful, if not the most remarkable, of all the properties of this extraordinary kind of air, is its power of preserving animal substances from putrefaction, and of restoring those that are already putrid, which it possesses in a far greater degree than fixed air. My first observation of this was altogether casual. Having found nitrous air to suffer so great a diminution as I have already mentioned by a mixture of iron filings and brimstone, I was willing to try whether it would be equally diminished by other causes of the diminution of common air, especially by putrefaction ; and for this purpose I put a dead mouse into a quantity of it, and placed it near the fire, where the tendency to putrefaction was very great. In this case there was a considerable diminution, *viz.* from $5\frac{1}{2}$ to $3\frac{1}{2}$; but not so great as I had expected, the antiseptic power of the nitrous air having checked the

the tendency to putrefaction; for when, after a week, I took the mouse out, I perceived, to my very great surprize, that it had no offensive smell.

Upon this I took two other mice, one of them just killed, and the other soft and putrid, and put them both into the same jar of nitrous air, standing in the usual temperature of the weather; in the months of July and August of 1772; and after 25 days, having observed that there was little or no change in the quantity of the air, I took the mice out; and, examining them, found them both perfectly sweet, even when cut through in all places. That which had been put into the air when just dead was quite firm; and the flesh of the other, which had been putrid and soft, was still soft, but perfectly sweet.

In order to compare the antiseptic power of this kind of air with that of fixed air, I examined a mouse which I had inclosed in a phial full of fixed air, as pure as I could make it, and which I had corked very close; but upon opening this phial in water, about a month after, I perceived that a large quantity of putrid effluviūm had been generated; for it rushed with violence out of the phial; and the smell that came from it, the moment the cork was taken out, was insufferably offensive. Indeed Dr. Macbride says, that he could only restore very thin pieces of putrid flesh by means of fixed air. Perhaps the antiseptic power of these kinds of air may be in proportion to their acidity. If a little pains were taken with this subject, this remarkable antiseptic power of nitrous air might possibly be applied to various uses, perhaps to the preservation

preservation of the more delicate birds, fishes, fruits, &c. mixing it in different proportions with common or fixed air. Of this property of nitrous air anatomists may perhaps avail themselves, as animal substances may by this means be preserved in their natural soft state; but how long it will answer for this purpose, experience only can shew.

I calcined lead and tin in the manner hereafter described in a quantity of nitrous air, but with very little sensible effect; which rather surprized me; as, from the result of the experiment with the iron filings and brimstone, I had expected a very great diminution of the nitrous air by this process, the mixture of iron filings and brimstone, and the calcination of metals, having the same effect upon common air, both of them diminishing it in nearly the same proportion.

Nitrous air is procured from all the proper metals by spirit of nitre, except lead, and from all the semi-metals that I have tried, except zinc. For this purpose I have used bitumeth and nickel, with spirit of nitre only, and regulus of antimony and platina, with aqua regia.

I got little or no air from lead by spirit of nitre, and have not yet made any experiments to ascertain the nature of this solution. With zinc I have taken a little pains.

Four penny weights and seventeen grains of zinc dissolved in spirit of nitre, to which as much water was added, yielded about twelve ounce measures of air, which had, in some degree, the properties of nitrous air, making a slight effervescence with common air, and diminishing it about as much as nitrous

trous air, which had been itself diminished one half by washing in water. The smell of them both was also the same; so that I concluded it to be the same thing, that part of the nitrous air which is imbibed by water being retained in this solution.

In order to discover whether this was the case, I made the solution boil in a sand heat. Some air came from it in this state, which seemed to be the same thing, as nitrous air diminished about one sixth, or one eighth, by washing in water. When the fluid part was evaporated, there remained a brown fixed substance, which was observed by Mr. Helot, who describes it, *Ac. Par.* 1735, *M.* p. 35. A part of this I threw into a small red hot crucible; and covering it immediately with a receiver, standing in water, I observed that very dense red fumes rose from it, and filled the receiver. This redness continued about as long as that which is occasioned by a mixture of nitrous and common air; the air was also considerably diminished within the receiver. This substance, therefore, must certainly have contained within it the very same thing, or principle, on which the peculiar properties of nitrous air depend. It is remarkable, however, that though the air within the receiver was diminished about one fifth by this process, it was itself as much affected with a mixture of nitrous air, as common air is, and a candle burnt in it very well. This may perhaps be attributed to some effect of the spirit of nitre, in the composition of that brown substance.

Nitrous air, I find, will be considerably diminished in its bulk by standing a long time in water,

ter, about as much as inflammable air is diminished in the same circumstances. For this purpose I kept for some months a quart bottle full of each of these kinds of air; but as different quantities of inflammable air vary very much in this respect, it is not improbable but that nitrous air may vary also.

From one trial that I made, I conclude that nitrous air may be kept in a bladder much better than most other kinds of air. The air to which I refer was kept about a fortnight in a bladder, through which the peculiar smell of the nitrous air was very sensible for several days. In a day or two the bladder became red, and was much contracted in its dimensions. The air within it had lost very little of its peculiar property of diminishing common air.

I did not endeavour to ascertain the exact quantity of nitrous air produced from given quantities of all the metals which yield it; but the few observations which I did make for this purpose I shall recite in this place:

dwt. gr.

6	0	of silver yielded	17 $\frac{1}{2}$	ounce measures
5	19	of quicksilver	4 $\frac{1}{2}$	
1	2 $\frac{1}{2}$	of copper	14 $\frac{1}{2}$	
2	0	of brass	21	
0	20	of iron	16	
1	5	of bismuth	6	
0	12	of nickel	4	

VII.

OF AIR INFECTED WITH THE FUMES OF BURNING CHARCOAL.

Air infected with the fumes of burning charcoal is well known to be noxious; and the Honourable Mr. Cavendish favoured me with an account of some experiments of his, in which a quantity of common air was reduced from 180 to 162 ounce measures, by passing through a red-hot iron tube filled with the dust of charcoal. This diminution he ascribed to such a destruction of common air as Dr. Hales imagined to be the consequence of burning. Mr. Cavendish also observed, that there had been a generation of fixed air in this process, but that it was absorbed by sope leys. This experiment I also repeated, with a small variation of circumstances, and with nearly the same result.

Afterwards, I endeavoured to ascertain, by what appears to me to be an easier and a more certain method, in what manner air is affected with the fumes of charcoal, viz. by suspending bits of charcoal within glass vessels, filled to a certain height with water, and standing inverted in another vessel of water, while I threw the focus of a burning mirror, or lens, upon them. In this manner I diminished a given quantity of air one fifth, which is nearly in the same proportion with other diminutions of air.

Some fixed air seems to be contained in charcoal, and to be set loose from it by this process; for if I made use of lime-water, it never failed to become

turbid, presently after the heat was applied. This was the case with whatever degree of heat the charcoal had been made. If, however, the charcoal had not been made with a very considerable degree of heat, there never failed to be a permanent addition of inflammable air produced; which agrees with what I observed before, that, in converting dry wood into charcoal, the greatest part is changed into inflammable air. I have sometimes found, that charcoal which was made with the most intense heat of a smith's fire, which vitrified part of a common crucible in which the charcoal was confined, and which had been continued above half an hour, did not diminish the air in which the focus of a burning mirror was thrown upon it; a quantity of inflammable air equal to the diminution of the common air being generated in the process; whereas, at other times, I have not perceived that there was any generation of inflammable air, but a perfect diminution of common air, when the charcoal had been made with a much less degree of heat. This subject deserves to be farther investigated.

To make the preceding experiment with still more accuracy, I repeated it in quicksilver; when I perceived that there was a small increase of the quantity of air, from a generation either of fixed or inflammable air, but I suppose of the former. Thus it stood without any alteration a whole night, and part of the following day; when lime-water, being admitted to it, it presently became turbid, and, after some time, the whole quantity of air, which was about four ounce measures, was diminished one fifth, as before. In this case, I carefully weighed the piece of charcoal, which was exactly two grains, and could not find that

that it was sensibly diminished in weight by the operation.

Air thus diminished by the fumes of burning charcoal not only extinguishes flame, but is in the highest degree noxious to animals; it makes no effervescence with nitrous air, and is incapable of being diminished any farther by the fumes of more charcoal, by a mixture of iron filings and brimstone, or by any other cause of the diminution of air that I am acquainted with.

This observation, which respects all other kinds of diminished air, proves that Dr. Hales was mistaken in his notion of the absorption of air in those circumstances in which he observed it. For he supposed that the remainder was, in all cases, of the same nature with that which had been absorbed, and that the operation of the same cause would not have failed to produce a farther diminution; whereas all my observations not only shew that air, which has once been fully diminished by any cause whatever, is not only incapable of any farther diminution, either from the same or from any other cause, but that it has likewise acquired new properties, most remarkably different from those which it had before, and that they are, in a great measure, the same in all the cases. These circumstances give reason to suspect, that the cause of diminution is, in reality, the same in all the cases. What this cause is, may, perhaps, appear in the next course of observations.

VIII.

OF THE EFFECT OF THE CALCINATION OF METALS, AND OF THE EFFLUVIA OF PAINT MADE WITH WHITE-LEAD AND OIL, ON AIR.

Having been led to suspect, from the experiments which I had made with charcoal, that the diminution of air in that case, and perhaps in other cases also, was, in some way or other, the consequence of its having more than its usual quantity of phlogiston, it occurred to me, that the calcination of metals, which are generally supposed to consist of nothing but a metallic earth united to phlogiston, would tend to ascertain the fact, and be a kind of *experimentum crucis* in the case. Accordingly, I suspended pieces of lead and tin in given quantities of air, in the same manner as I had before treated the charcoal; and throwing the focus of a burning mirror or lens upon them, in such a manner as to make them fume copiously, I presently perceived a diminution of the air. In the first trial that I made, I reduced four ounce measures of air to three, which is the greatest diminution of common air that I had ever observed before, and which I account for, by supposing that, in other cases, there was not only a cause of diminution, but causes of addition also, either of fixed or inflammable air, or some other permanently elastic matter, but that, the effect of the calcination of metals being simply the escape of phlogiston, the cause of diminution was alone and uncontrouled.

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The air, which I had thus diminished by calcination of lead, I transferred into another clean phial, but found that the calcination of more lead in it had no farther effect upon it. This air also, like that which had been infected with the fumes of charcoal, was in the highest degree noxious, made no effervescence with nitrous air, was no farther diminished by the mixture of iron filings and brimstone, and was not only rendered innoxious, but also recovered, in a great measure, the other properties of common air, by washing in water.

It might be suspected that the noxious quality of the air in which lead was calcined, might be owing to some fumes peculiar to that metal; but I found no sensible difference between the properties of this air, and that in which tin was calcined.

The water over which metals are calcined acquires a yellowish tinge, and an exceedingly pungent smell and taste, pretty much, as near as I can recollect, for I did not compare them together, like that over which brimstone has been frequently burned. Also a thin and whitish pellicle covered both the surface of the water, and likewise the sides of the phial in which the calcination was made, insomuch that, without frequently agitating the water, it grew so opaque by this constantly accumulating incrustation, that the sun beams could not be transmitted through it in a quantity sufficient to produce the calcination.

I imagined, however, that, even when this air was transferred into a clean phial, the metals were not so easily melted or calcined as they were in fresh air; for the air being once fully saturated with phlogiston, may not so readily admit any more, though it be only
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to transmit it to the water. I also suspected that metals were not easily melted or calcined in inflammable, fixed, or nitrous, air, or any kind of diminished air. None of these kinds of air suffered any change by this operation; nor was there any precipitation of lime, when charcoal was heated in any of these kinds of air standing in lime-water.

Query. May not water impregnated with phlogiston from calcined metals, or by any other method, be of some use in medicine? The effect of this impregnation is exceedingly remarkable; but the principle with which it is impregnated is volatile, and entirely escapes in a day or two, if the surface of the water be exposed to the common atmosphere.

It should seem that phlogiston is retained more obstinately by charcoal than it is by lead or tin; for when any given quantity of air is fully saturated with phlogiston from charcoal, no heat that I have yet applied has been able to produce any more effect upon it; whereas, in the same circumstances, lead and tin may still be calcined. The air, indeed, can take no more; but the water receives it, and the sides of the phial also receive an addition of incrustation. This is a white powdery substance, and well deserves to be examined. I shall endeavour to do it at my leisure.

Lime-water never became turbid by the calcination of metals over it; but the colour, smell, and taste of the water was always changed, and the surface of it became covered with a yellow pellicle, as before.

When this process was made in quicksilver, the air was diminished only one fifth; and upon water being

admitted to it, no more was absorbed; which is an effect similar to that of a mixture of nitrous and common air, which was mentioned before.

The preceding experiments on the calcination of metals suggested to me a method of explaining the cause of the mischief which is known to arise from fresh paint, made with white lead (which I suppose is an imperfect calx of lead) and oil. To verify my hypothesis, I first put a small pot full of this kind of paint, and afterwards (which answered much better, by exposing a greater surface of the paint) I daubed several pieces of paper with it, and put them under a receiver, and observed, that in about twenty-four hours, the air was diminished between one fifth and one fourth, for I did not measure it very exactly. This air also was, as I expected to find it, in the highest degree, noxious; it did not effervesce with nitrous air, it was no farther diminished by a mixture of iron filings and brimstone, and was made wholesome by agitation in water deprived of all air.

I think it appears pretty evident, from the preceding experiments on the calcination of metals, that air is some way or other diminished in consequence of being highly charged with phlogiston, and that agitation in water restores it, by imbibing a great part of the phlogistic matter. That water has a considerable affinity with phlogiston, is evident from the strong impregnation which it receives from it. May not plants also restore air diminished by putrefaction, by absorbing part of the phlogiston with which it is loaded? The greater part of a dry plant, as well as of a dry animal substance, consists of inflammable air, or something that is capable of being converted
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into inflammable air ; and it seems to be as probable that this phlogistic matter may have been imbibed by the roots and leaves of plants, and afterwards incorporated into their substance, as that it is altogether produced by the power of vegetation. May not this phlogistic matter be even the most essential part of the food and support of both vegetable and animal bodies ?

In the experiments with metals, the diminution of air seems to be the consequence of nothing but a saturation with phlogiston ; and in all the other cases of the diminution of air, I do not see but that it may be effected by the same means. When a vegetable or animal substance is dissolved by putrefaction, the escape of the phlogistic matter (which, together with all its other constituent parts, is then let loose from it) may be the circumstance that produces the diminution of the air in which it putrefies. It is highly improbable that what remains after an animal body has been thoroughly dissolved by putrefaction, should yield so great a quantity of inflammable air, as the dried animal substance would have done. Of this I have not made an actual trial, though I have often thought of doing it, and still intend to do it ; but I think there can be no doubt of the result. Again, the iron, by its fermentation with brimstone and water, is evidently reduced to a calx, so that phlogiston must have escaped from it. Phlogiston also must evidently be set loose by the ignition of charcoal, and is not improbably the matter which flies off from paint, composed of white lead and oil. Lastly, since spirit of nitre is known to have a very remarkable affinity with phlogiston, it is far from
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being improbable that nitrous air may also produce the same effect by the same means.

To this hypothesis it may be objected, that, if diminished air be air saturated with phlogiston, it ought to be inflammable; but this by no means follows, since its inflammability may depend upon some particular mode of combination, or degree of affinity, with which we are not acquainted. Besides, inflammable air seems to consist of some other principle, or to have some other constituent part; besides phlogiston and common air, as is probable from that remarkable deposit, which, as I have observed, is made by inflammable air, both from iron and zinc.

It is not improbable, however, but that a greater degree of heat may inflame that air which extinguishes a common candle, if it could be conveniently applied. Air that is inflammable, I observe, extinguishes red hot wood; and indeed inflammable substances can only be those which, in a certain degree of heat, have a less affinity with the phlogiston they contain, than the air, or some other contiguous substance, has with it; so that the phlogiston only quits one substance, with which it was before combined, and enters another, with which it may be combined in a very different manner. This substance, however, whether it be air or any thing else, being now fully saturated with phlogiston, and not being able to take any more, in the same circumstances, must necessarily extinguish fire, and put a stop to the ignition of all other bodies, that is, to the farther escape of phlogiston from them.

That plants restore noxious air, by imbibing the phlogiston with which it is loaded, is very agreeable to

the conjectures of Dr. Franklin, made many years ago, and expressed in the following extract from the last edition of his Letters, p. 346.

“ I have been inclined to think that the fluid *fire*,
 “ as well as the fluid *air*, is attracted by plants in
 “ their growth, and becomes consolidated with the
 “ other materials of which they are formed, and
 “ makes a great part of their substance; that, when
 “ they come to be digested, and to suffer in the
 “ vessels a kind of fermentation, part of the fire, as
 “ well as part of the air, recovers its fluid active state
 “ again, and diffuses itself in the body, digesting and
 “ separating it; that the fire so reproduced, by di-
 “ gestion and separation, continually leaving the
 “ body, its place is supplied by fresh quantities,
 “ arising from the continual separation; that what-
 “ ever quickens the motion of the fluids in an ani-
 “ mal quickens the separation, and re-produces
 “ more of the fire, as exercise; that all the fire
 “ emitted by wood, and other combustibles, when
 “ burning, existed in them before, in a solid state,
 “ being only discovered when separating; that some
 “ fossils, as sulphur, sea-coal, &c. contain a great
 “ deal of solid fire; and that, in short, what escapes
 “ and is dissipated in the burning of bodies, besides
 “ water and earth, is generally the air, and fire,
 “ that before made parts of the solid.”

IX.

OF AIR PROCURED BY MEANS OF SPIRIT OF SALT.

Being very much struck with the result of an experiment of the Hon. Mr. Cavendish, related Phil. Trans.

Transf. Vol. LVI. p. 157. by which, though, he says, he was not able to get any inflammable air from copper, by means of spirit of salt, he got a much more remarkable kind of air, *viz.* one that lost its elasticity by coming into contact with water, I was exceedingly desirous of making myself acquainted with it. On this account, I began with making the experiment in quicksilver, which I never failed to do in any case in which I suspected that air might either be absorbed by water, or be in any other manner affected by it; and by this means I presently got a much more distinct idea of the nature and effects of this curious solution.

Having put some copper filings into a small phial, with a quantity of spirit of salt; and making the air, which was generated in great plenty, on the application of heat, ascend into a tall glass vessel full of quicksilver, and standing in quicksilver, the whole produce continued a considerable time without any change of dimensions. I then introduced a small quantity of water to it, when about three fourths of it (the whole being about four ounce measures) presently, but gradually, disappeared, the quicksilver rising in the vessel. I then introduced a considerable quantity of water; but there was no farther diminution of the air, and the remainder I found to be inflammable.

Having frequently continued this process a long time after the admission of the water, I was much amused with observing the large bubbles of the newly generated air, which came through the quicksilver, the sudden diminution of them when they came to the water, and the very small bubbles which went

through the water. They made, however, a continual, though slow, increase of inflammable air.

Fixed air, being admitted to the whole produce of this air from copper, had no sensible effect upon it. Upon the admission of water, a great part of the mixture, which, no doubt, was the most subtle kind of air from the copper, presently disappeared; another part, which I suppose to have been the fixed air, was absorbed slowly; and in this particular case the very small permanent residuum did not take fire; but it is very possible that it might have done so, if the quantity had been greater.

Lime-water being admitted to the whole produce of air from copper became white; but this I suspect to have arisen from some other circumstance than the precipitation of the lime which it contained.

The solution of lead in the marine acid is attended with the very same phenomena as the solution of copper in the same acid; about three fourths of the generated air disappearing on the contact of water, and the remainder being inflammable.

The solutions of iron, tin, and zinc, in the marine acid; were all attended with the same phenomena as the solutions of copper and lead, but in a less degree; for in iron one eighth, in tin one sixth, and in zinc one tenth of the generated air disappeared on the contact with water. The remainder of the air from iron, in this case, burned with a green, or very light blue flame.

I had always thought it something extraordinary that a species of air should lose its elasticity by the mere contact of any thing, and from the first suspected that it must have been imbibed by the water
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that was admitted to it; but so very great a quantity of this air disappeared upon the admission of a very small quantity of water, that I could not help concluding that appearances favoured the former hypothesis. I found, however, that when I admitted a much smaller quantity of water, confined in a narrow glass tube, a part only of the air disappeared, and that very slowly, and that more of it vanished upon the admission of more water. This observation put it beyond a doubt, that this air was properly imbibed by the water, which, being once fully saturated with it, was not capable of receiving any more. The water thus impregnated tasted very acid, even when it was much diluted with other water, through which the tube containing it was drawn. It even dissolved iron very fast, and generated inflammable air. This last observation, together with another which immediately follows, led me to the discovery of the true nature of this remarkable kind of air, as it had hitherto been called.

Happening, at one time, to use a good deal of copper and a small quantity of spirit of salt, in the generation of this kind of air, I was surprized to find that air was produced long after, I could not but think that the acid must have been saturated with the metal; and I also found that the proportion of inflammable air to that which was absorbed by the water continually diminished, till, instead of being one fourth of the whole as I had first observed, it was not so much as one twentieth. Upon this, I concluded that this subtle air did not arise from the copper, but from the spirit of salt; and presently making the experiment with the acid only, without any cop-

per, or metal of any kind, this air was immediately produced in as great plenty as before; so that this remarkable kind of air is, in fact, nothing more than the vapour, or fumes of spirit of salt, which appear to be of such a nature, that they are not liable to be condensed by cold, like the vapour of water, and other fluids. This vapour, however, seems to lose its elasticity, in some measure, gradually, unless it should be thought to be affected by the quicksilver, with which it is in contact; for it was always diminished, more or less, by standing.

This elastic acid vapour extinguishes flame, and is much heavier than common air; but how much heavier, will not be easy to ascertain. A cylindrical glass vessel, about three fourths of an inch in diameter, and four inches deep, being filled with it, and turned upside down, a lighted candle may be let down into it more than twenty times before it will burn at the bottom. It is pleasing to observe the colour of the flame in this experiment; for both before the candle goes out, and also when it is first lighted again, it burns with a beautifully green, or rather light blue flame, such as is seen when common salt is thrown into the fire.

When this elastic vapour is all expelled from any quantity of spirit of salt, which is easily perceived by the vapour being condensed by cold, the remainder is a very weak acid, barely capable of dissolving iron.

Being now in the possession of a new subject of experiments, *viz.* an elastic acid vapour, in the form of a permanent air, easily procured, and effectually confined by glass and quicksilver, with
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which it did not seem to have any affinity; I immediately began to introduce a variety of substances to it, in order to ascertain its peculiar properties and affinities, and also the properties of those other bodies with respect to it.

Beginning with water, which, from preceding observations, I knew would imbibe it, and become impregnated with it; I found that $2\frac{1}{2}$ grains of rain water absorbed three ounce measures of this vapour, after which it was increased one third in its bulk, and weighed twice as much as before; so that this concentrated vapour seems to be twice as heavy as rain water. Water impregnated with it makes the strongest spirit of salt that I have seen, dissolving iron with the most rapidity. Consequently, two thirds of the best spirit of salt is nothing more than mere phlegm or water.

Iron filings, being admitted to this vapour, were dissolved by it pretty fast, half of the vapour disappearing, and the other half becoming inflammable air, not absorbed by water. Putting chalk to it, fixed air was produced.

I had not introduced many substances to this vapour, before I discovered that it had an affinity with phlogiston, so that it would deprive other substances of it, and form with it such an union as constitutes inflammable air; which seems to shew, that inflammable air universally consists of the union of some acid vapour with phlogiston.

Inflammable air was produced, when to this vapour I put spirit of wine, oil of olives, oil of turpentine, charcoal, phosphorus, bees-wax, and even sulphur. This last observation, I own, surprised!

prized me ; for, the marine acid being reckoned the weakest of the three mineral acids, I did not think that it had been capable of discharging the oil of vitriol from this substance; but I found that it had the very same effect both upon alum and nitre; the vitriolic acid in the former case, and the nitrous in the latter, giving place to the stronger vapour of spirit of salt.

The rust of iron, and the precipitate of nitrous air made from copper, also imbibed this vapour very fast, and the little that remained of it was inflammable air; which proves, that these calces contain phlogiston. It seems also to be pretty evident, from this experiment, that the precipitate above-mentioned is a real calx of the metal, by the solution of which the nitrous air is generated.

As some remarkable circumstances attend the absorption of this vapour of spirit of salt, by the substances above-mentioned, I shall briefly mention them.

Spirit of wine absorbs this vapour as readily as water itself, and is increased in bulk by that means. Also, when it is saturated, it dissolves iron with as much rapidity, and still continues inflammable.

Oil of olives absorbs this vapour very slowly, and, at the same time, it turns almost black, and becomes glutinous. It is also less miscible with water, and acquires a very disagreeable smell. By continuing upon the surface of the water, it became white, and its offensive smell went off in a few days.

Oil of turpentine absorbed this vapour very fast, turning brown, and almost black. No inflammable air was formed, till I raised more of the vapour than the
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the oil was able to absorb, and let it stand a considerable time; and still the air was but weakly inflammable. The same was the case with the oil of olives, in the last mentioned experiment; and it seems to be probable, that, the longer this acid vapour had continued in contact with the oil, the more phlogiston it would have extracted from it. It is not improbable, but that, in the intermediate state, before it becomes inflammable air, it may be nearly of the nature of common air.

Bees-wax absorbed this vapour very slowly. About the bigness of a hazel-nut of the wax being put to three ounce measures of the vapour, the vapour was diminished one half in two days, and, upon the admission of water, half of the remainder also disappeared. This air was strongly inflammable.

Charcoal absorbed this vapour very fast. About one fourth of it was rendered immiscible in water, and was but weakly inflammable.

A small bit of phosphorus, perhaps about half a grain, smoked, and gave light in the vapour of spirit of salt, just as it would have done in common air confined. It was not sensibly wasted after continuing about twelve hours in that state, and the bulk of the vapour was very little diminished. Water being admitted to it absorbed it as before, except about one fifth of the whole, which was but weakly inflammable.

Putting several pieces of sulphur to this vapour, it was absorbed but slowly. In about twenty-four hours about one fifth of the quantity had disappeared; and water being admitted to the remainder, very little

more was absorbed. The remainder was inflammable, and burned with a blue flame.

Nowwithstanding the affinity which this vapour of spirit of salt appears to have with phlogiston, it is not capable of depriving all bodies of it. I found that dry wood, crusts of bread, and raw flesh, very readily imbibed this acid vapour, but did not part with any of their phlogiston to it. All these substances turned very brown, after they had been some time exposed to this vapour, and tailed very strongly of the acid when they were taken out; but the flesh, when washed in water, became very white, and the fibres easily separated from one another, even more than they would have done if it had been boiled or roasted.

When I put a piece of saltpetre to this vapour, it was presently surrounded with a white fume, which soon filled the whole vessel, exactly like the fume which bursts from the bubbles of nitrous air, when it is generated by a vigorous fermentation, and such as is seen when nitrous air is mixed with this vapour of spirit of salt. In about a minute, the whole quantity of vapour was absorbed, except a very small quantity, which might be the common air that had lodged upon the surface of the spirit of salt within the phial.

A piece of alum exposed to this vapour turned yellow, absorbed it as fast as the saltpetre had done, and was reduced by it to the form of a powder. The surface both of the nitre and alum was, I doubt not, changed into common salt, by this process. Common salt, as might be expected, had no effect whatever on this vapour.

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From considering the affinity which this vapour has with phlogiston, I was induced to try the effect of a mixture of it with nitrous air. Accordingly, to two parts of this vapour, I put one part of nitrous air, and, in about twenty-four hours, the whole was diminished to something less than the original quantity of the vapour, and was no farther diminished by the admission of water. Holding the flame of a candle over this air, the lower part of it burned green, but there was no sensible explosion. At different times I collected $2\frac{3}{4}$ ounce measures of this mixture of air; but, upon agitating it in rain-water, it was presently diminished to $1\frac{1}{2}$ ounce measures. In this state it effervesced with nitrous air, and was considerably diminished by it, but not so much as common air. Some allowance, no doubt, must be made for the small quantities of common air, which lodged on the top of my phials, when I raised the fume from the spirit of salt; but, from the precautions that I made use of, I think that very little is to be allowed to this circumstance; and, upon the whole, I am of opinion, that this experiment is an approach to the generation of common air, or air fit for respiration.

I had also imagined, that if air diminished by the processes above-mentioned was affected in this manner, in consequence of its being saturated with phlogiston, a mixture of this vapour might imbibe that phlogiston, and render it wholesome again; but I put about one fourth of this vapour to a quantity of air in which metals had been calcined, without making any sensible alteration in it. I do not, however, infer from this, that air is not diminished by means of phlogiston, since the air, like some other substances,

may hold the phlogiston too fast, to be deprived of it by this acid vapour.

I shall conclude my account of these experiments with observing, that the electric spark is visible in the vapour of spirit of salt, exactly as it is in common air; and though I kept making this spark a considerable time in a quantity of it, I did not perceive that any sensible alteration was made in it. A little inflammable air was produced, but not more than might have come from the two iron nails which I made use of in taking the sparks.

X.

MISCELLANEOUS OBSERVATIONS.

Many of the preceding observations relating to the vinous and putrefactive fermentations, I had the curiosity to endeavour to ascertain in what manner the air would be affected by the acetous fermentation. For this purpose I inclosed a phial full of small beer in a jar standing in water, and observed that during the first two or three days there was an increase of the air in the jar, but from that time it gradually decreased, till at length there appeared to be a diminution of about $\frac{1}{10}$ of the whole quantity. During this time the whole surface of it was gradually covered with a scum, beautifully corrugated. After this there was an increase of the air till there was more than the original quantity; but this must have been fixed air, not incorporated with the rest of the mass; for, withdrawing the beer, which I found to be sour, after it had stood 18 or 20 days under the jar, and
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passing the air several times through cold water, the original quantity was diminished about $\frac{1}{7}$. In the remainder a candle would not burn, and a mouse would have died presently. The smell of this air was exceedingly pungent, but different from that of the putrid effluvium. A mouse lived perfectly well in this air, thus affected with the acetous fermentation; after it had stood several days mixed with four times the quantity of fixed air.

All the kinds of factitious air on which I have yet made the experiment are highly noxious to animals, except that which is extracted from salt-petre, or alum; but in this even a candle burned just as in common air. In one quantity which I got from salt-petre a candle not only burned, but the flame was increased, and something was heard like a hissing, similar to the decrepitation of nitre in an open fire. This experiment was made when the air was fresh made, and while it probably contained some particles of nitre, which would have been deposited afterwards. The air was extracted from these substances by putting them into a gun barrel, which was much corroded and soon spoiled by the experiment. What effect this circumstance may have had upon the air I have not considered.

November 6, 1772, I had the curiosity to examine the state of a quantity of this air, which had been extracted from salt-petre above a year, and which at first was perfectly wholesome; when, to my very great surprize, I found that it was become, in the highest degree, noxious. It made no effervescence with nitrous air, and a mouse died the moment it was put into it. I had not, however, washed it in rain water quite ten minutes
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(and perhaps less time would have been sufficient) when I found, upon trial, that it was restored to its former perfectly wholesome state. It effervesced with nitrous air as much as the best common air ever does, and even a candle burned in it very well, which I had never before observed of any kind of noxious air meliorated by agitation in water. This series of facts, relating to air extracted from nitre, appear to me to be very extraordinary and important, and, in able hands, may lead to considerable discoveries.

There are many substances which impregnate the air in a very remarkable manner, but without making it noxious to animals. Among other things I tried volatile alkaline salts, and camphire, the latter of which I melted with a burning glass, in air inclosed in a phial. The mouse which was put into this air sneezed and coughed very much, especially after it was taken out; but it presently recovered, and did not appear to have been sensibly injured.

Having made several experiments with a mixture of iron filings and brimstone, kneaded to a paste with water, I had the curiosity to try what would be the effect of substituting brass dust in the place of the iron filings. The result was, that when this mixture had stood about three weeks, in a given quantity of air, it had turned black, but was not increased in bulk. The air also was neither sensibly increased nor decreased, but the nature of it was changed, for it extinguished flame, it would have killed a mouse presently, and was not restored by fixed air, which had been mixed with it several days.

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I have frequently mentioned my having, at one time, exposed equal quantities of different kinds of air in jars standing in boiled water. The common air in this experiment was diminished four sevenths, and the remainder extinguished flame. This experiment demonstrates that water does not absorb air equally, but that it decomposes it, taking one part, and leaving the rest. To be quite sure of this fact, I agitated a quantity of common air in boiled water, and when I had reduced it from eleven ounce measures to seven, I found that it extinguished a candle, but a mouse lived in it very well. At another time a candle barely went out when the air was diminished one third, and at other times I have found this effect take place at other very different degrees of diminution. This difference I attribute to the differences in the state of the water with respect to the air contained in it; for sometimes it had stood longer than at other times before I made use of it. I also used distilled water, rain water, and water out of which the air had been pumped, promiscuously with rain water. I even doubt not but that, in a certain state of the water, there might be no sensible difference in the bulk of the agitated air, and yet at the end of the process it would extinguish a candle, air being supplied from the water in the place of that part of the common air which had been absorbed.

It is certainly a little extraordinary that the very same process should so far mend putrid air, as to reduce it to the standard of air in which candles have burned out; and yet that it should so far injure common and wholesome air, as to reduce it to about
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the same standard: but to the fact certainly is. If air extinguish flame in consequence of its being previously saturated with phlogiston, it must, in this case, have been transferred from the water to the air.

To a quantity of common air, thus diminished by agitation in water, till it extinguished a candle, I put a plant, but it did not so far restore it as that a candle would burn in it again; which to me appeared not a little extraordinary, as it did not seem to be in a worse state than air in which candles had burned out, and which had never failed to be restored by the same means. I had no better success with a quantity of permanent air; which I had collected from my pump water. Indeed these experiments were begun before I was acquainted with that property of nitrous air, which makes it so accurate a measure of the goodness of other kinds of air; and it might perhaps be rather too late in the year when I made the experiments. Having neglected these two jars of air, the plants died and putrefied in both of them; and then I found the air in them both to be highly noxious, and to make no effervescence with nitrous air.

I found that a pint of my pump water contains about one fourth of an ounce measure of air, one half of which was afterwards absorbed by standing in fresh pump water. A candle would not burn in the air, but a mouse lived in it very well. Upon the whole, it seemed to be in about the same state as air in which a candle had burned out.

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I once imagined that, by mere stagnation, air might become unfit for respiration, or at least for the burning of candles; but if this be the case, and the change be produced gradually, it must require a long time for the purpose. For on the 22d of September 1772, I examined a quantity of common air, which had been kept in a phial, without agitation, from May 1771, and found it to be in no respect worse than fresh air, even by the test of the nitrous air.

The crystallization of nitre makes no sensible alteration in the air in which the process is made. For this purpose I dissolved as much nitre as a quantity of hot water would contain, and let it cool under a receiver, standing in water.

November 6, 1772, a quantity of inflammable air, which, by long keeping, had come to extinguish flame, I observed to smell very much like common air in which a mixture of iron filings and brimstone had stood. It was not, however, quite so strong, but it was equally noxious.

Bismuth and nickel are dissolved in the marine acid with the application of a considerable degree of heat; but little or no air is got from either of them; but, what I thought a little remarkable, both of them smelled very much like Harrowgate water. This smell I have met with several times in the course of my experiments, and in processes very different from one another.

As I generally made use of mice in the experiments which relate to respiration, and some persons may chuse to repeat them after me, and pursue them farther than I have done; it may be

of use to them to be informed, that I kept them without any difficulty in glass receivers, open at the top and bottom, and having a quantity of paper, or tow, in the inside, which should be changed every three or four days; when it will be most convenient also to change the vessel, and wash it. But they must be kept in a pretty exact temperature, for either much heat or much cold kills them presently. The place in which I have generally kept them is a shelf over the kitchen fire place, where, as it is usual in Yorkshire, the fire never goes out; so that the heat varies very little; and I find it to be at a medium about 70 degrees of Fahrenheit's thermometer. When they had been made to pass through the water, as they necessarily must be, in order to a change of air, they require, and will bear a very considerable degree of heat, to warm and dry them.

I found, to my great surprize, in the course of these experiments, that mice will live intirely without water; for though I have kept some of them for three or four months, and have offered them water several times, they would never taste it; and yet they continued in perfect health and vigour. Two or three of them will live very peaceably together in the same vessel; though I had one instance of one mouse tearing another almost in pieces, though there was plenty of provisions for both of them.

The apparatus with which the principal of the preceding experiments were made is exceedingly simple, and cheap. The drawing annexed (TAB. IX.) exhibits a view of every thing that is most important in it.

A is an oblong trough, about eight inches deep, kept nearly full of water, and B, B are jars standing in it, about ten inches long, and two and a half wide; such as I have generally used for electrical batteries.

C, C are flat stones, sunk about an inch, or half an inch, under the water, on which vessels of any kind may be conveniently placed, during a course of experiments.

D, D are pots nearly full of water, in which jars or phials, containing any kind of air, to which plants or any other substances may be exposed, and having their mouths immersed in water; so that the air in the inside can have no communication with the external air.

E is a small glass vessel, of a convenient size for putting a mouse into it, in order to try the wholesomeness of any kind of air that it may contain.

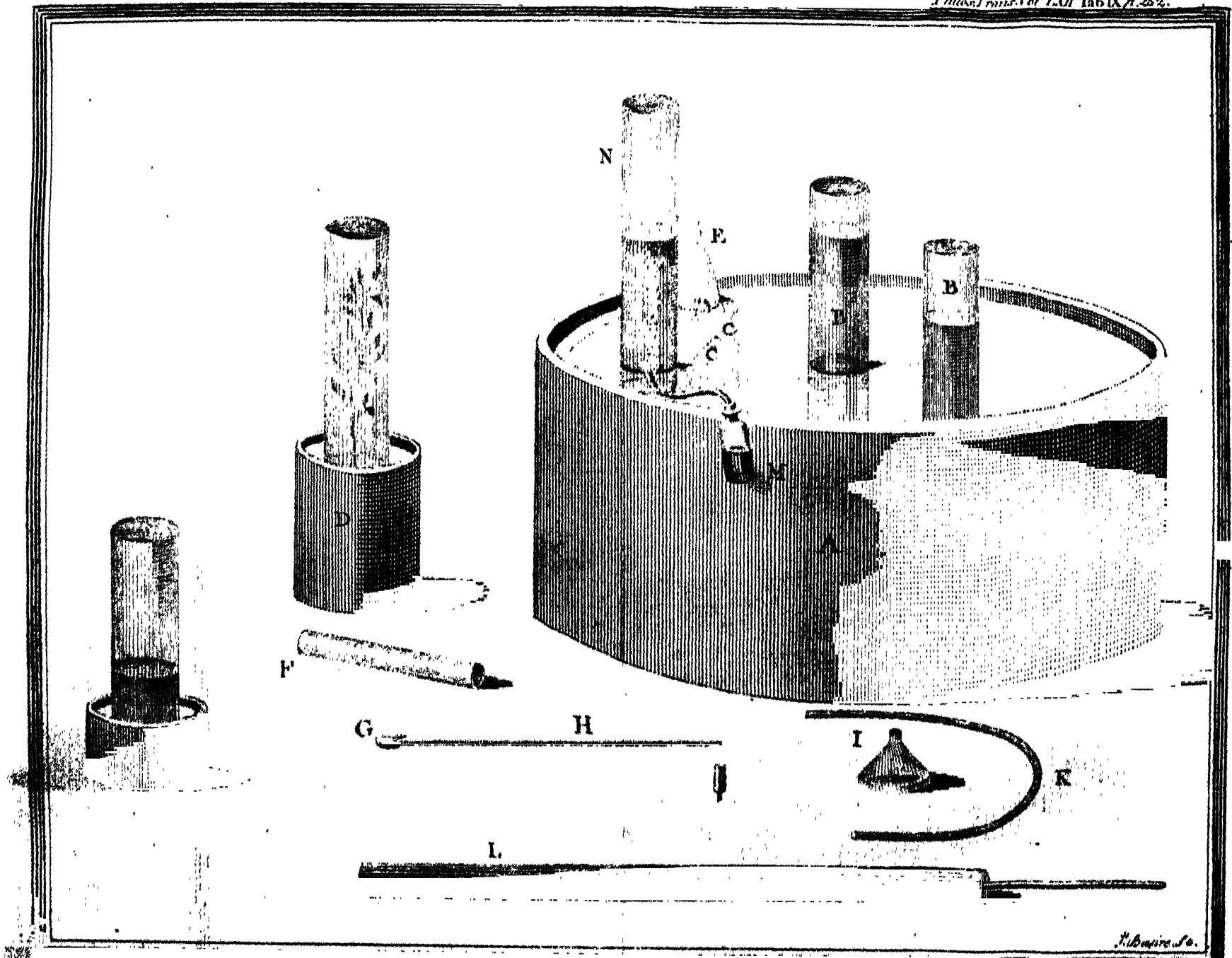
F is a cylindrical glass vessel, five inches in length, and one in diameter, very proper for trying whether any kind of air will admit a candle to burn in it. For this purpose a bit of wax candle, G, may be fastened to the end of a wire, H, and turned up in such a manner as to be let down into the vessel with the flame upwards. The vessel should be kept carefully covered till the moment that the candle is admitted to it. In this manner I have frequently extinguished a candle above twenty times in one of these vessels full of air, though it is impossible to dip the candle into it, without giving the external air an opportunity of mixing with it, more or less.

I is a funnel of glass or tin, which is necessary for transferring air into vessels which have narrow mouths.

K is a glass syphon, which is very useful for drawing air out of a vessel which has its mouth immersed in water, and thereby raising the water to whatever height may be most convenient. I do not think it by any means safe to depend upon a valve at the top of a vessel, which Dr. Hales very often made use of; for, since my first disappointments, I have never thought the communication between the external and internal air sufficiently cut off, unless glass, or a body of water, or, in some cases, quicksilver, have intervened between them.

L is a piece of a gun barrel, closed at one end, having the stem of a tobacco-pipe luted to the other. To the end of this pipe I sometimes fastened a flaccid bladder, in order to receive the air discharged from the substance contained in the barrel; but, when the air was generated slowly, I commonly contrived to put this end of the pipe under a vessel full of water, and standing with its mouth inverted in another vessel of water, that the new air might have a more perfect separation from the external air than a bladder could make.

M is a small phial containing some mixture that will generate air. This air passes through a bent glass tube inserted into the cork at one end, and going under the edge of the jar N at the other; the jar being placed with part of its mouth projecting beyond the flat stones C C for that purpose.



A N A P P E N D I X,

Containing an account of some experiments made by Mr. Hey, which prove that there is no oil of vitriol in water impregnated with fixed air extracted from chalk by oil of vitriol ; and also a letter from Mr. Hey, to Dr. Priestley, concerning the effects of fixed air applied by way of clyster.

EXPERIMENTS TO PROVE THAT THERE IS NO
OIL OF VITRIOL IN WATER IMPREGNATED
WITH FIXED AIR.

It having been suggested, that air arising from a fermenting mixture of chalk and oil of vitriol might carry up with it a small portion of the vitriolic acid, rendered volatile by the act of fermentation ; I made the following experiments, in order to discover whether the acidulous taste, which water impregnated with such air affords, was owing to the presence of any acid, or only to the fixed air it had absorbed.

EXPERIMENT I.

I mixed a tea-spoonful of syrup of violets with an ounce of distilled water, saturated with fixed air procured from chalk by means of the vitriolic acid ; but neither upon the first mixture, nor after
2 .. standing

standing 24 hours, was the colour of the syrup at all changed, except by its simple dilution.

EXPERIMENT II.

A portion of the same distilled water, unimpregnated with fixed air, was mixed with the syrup in the same proportion: not the least difference in colour could be perceived betwixt this and the above mentioned mixture.

EXPERIMENT III.

One drop of oil of vitriol being mixed with a pint of the same distilled water, an ounce of this water was mixed with a tea-spoonful of the syrup. This mixture was very distinguishable in colour from the two former, having a purplish cast, which the others wanted.

EXPERIMENT IV.

The distilled water impregnated with so small a quantity of vitriolic acid having a more agreeable taste than when alone, and yet manifesting the presence of an acid by means of the syrup of violets; I subjected it to some other tests of acidity. It formed curds when agitated with soap, lathered with difficulty, and very imperfectly; but not the least ebullition could be discovered upon dropping in spirit of sal ammoniac, or solution of salt of tartar, though I had taken care to render the latter free from causticity by impregnating it with fixed air.

Ex-

EXPERIMENT V.

The distilled water saturated with fixed air neither effervesced, nor shewed any clouds, when mixed with the fixed or volatile alkali.

EXPERIMENT VI.

No curd was formed by pouring this water upon an equal quantity of milk, and boiling them together.

EXPERIMENT VII.

When agitated with soap, this water produced curds, and lathered with some difficulty; but not so much as the distilled water mixed with vitriolic acid in the very small proportion above mentioned. The same distilled water without any impregnation of fixed air lathered with soap without the least previous curdling. River water, and a pleasant pump water not remarkably hard, were compared with these. The former produced curds before it lathered, but not quite in so great a quantity as the distilled water impregnated with fixed air: the latter caused a stronger curd than any of the others above-mentioned.

EXPERIMENT VIII.

Apprehending that the fixed air in the distilled water occasioned the coagulation, or separation of the oily part of the soap, only by destroying the causticity of the *lixivium*, and thereby rendering the

union less perfect betwixt that and the tallow, and not by the presence of any acid; I impregnated a fresh parcel of the same distilled water with fixed air, which had passed through half a yard of a wide barometer tube filled with salt of tartar; but this water caused the same curdling with soap as the former had done, and appeared in every respect to be exactly the same.

EXPERIMENT IX.

Distilled water saturated with fixed air formed a white cloud and precipitation, upon being mixed with a solution of *saccharum saturni*. I found likewise, that fixed air, after passing through the tube filled with alkaline salt, upon being let into a phial containing a solution of the metallic salt in distilled water, caused a perfect separation of the lead, in form of a white powder; for the water, after this precipitation, shewed no cloudiness upon a fresh mixture of the substances which had before rendered it opaque.

A Letter from Mr. HEY to Dr. PRIESTLEY, concerning the Effects of fixed Air applied by way of Clyster.

Leeds, Feb. 15th, 1772.

Reverend Sir,

Having lately experienced the good effects of fixed air in a putrid fever, applied in a manner, I believe, not heretofore made use of, I thought it proper to inform you of the agreeable event, as the method of applying this powerful corrector of putrefaction took its rise principally from your observations and experiments on factitious air; and now, at your request, I send the particulars of the case I mentioned to you, as far as concerns the administration of this remedy.

January 8, 1772, Mr. Lightbowne, a young gentleman who lives with me, was seized with a fever, which, after continuing about ten days, began to be attended with those symptoms that indicate a putrescent state of the fluids.

18th, His tongue was black in the morning when I first visited him, but the blackness went off in the day-time upon drinking: He had begun to doze much the preceding day, and now he took little notice of those that were about him: His belly was loose, and had been so for some days: his pulse beat 110 strokes in a minute, and was rather low: he was ordered to take twenty five grains of Peruvian bark with five of tormentill root in powder every four hours, and to use red wine and water cold as his common drink.

19th, I was called to visit him early in the morning, on account of a bleeding at the nose which had come on: he lost about eight ounces of blood, which was of a loose texture: the hæmorrhage was suppressed, though not without some difficulty, by means of tents made of soft lint, dipped in cold water strongly impregnated with tincture of iron, which were introduced within the nostrils quite through to their posterior apertures; a method which has never yet failed me in like cases. His tongue was now covered with a thick black pellicle, which was not diminished by drinking: his teeth were furred with the same kind of sordid matter, and even the roof of his mouth and fauces were not free from it: his looseness and stupor continued, and he was almost incessantly muttering to himself: he took this day a scruple of the Peruvian bark with ten grains of tormentill every two or three hours: a starch clyster containing a drachm of the compound powder of bole, without opium, was given morning and evening: a window was set open in his room, though it was a severe frost, and the floor was frequently sprinkled with vinegar.

20th, He continued nearly in the same state: when roused from his dozing, he generally gave a sensible answer to the questions asked him; but he immediately relapsed, and repeated his muttering. His skin was dry, and harsh, but without *petechiæ*. He sometimes voided his urine and *feces* into the bed, but generally had sense enough to ask for the bed-pan: as he now nauseated the bark in substance, it was exchanged for Huxham's tincture,

tincture, of which he took a table-spoonful every two hours in a cup full of cold water: he drank sometimes a little of the tincture of roses, but his common liquors were red wine and water, or rice water and brandy acidulated with elixir of vitriol: before drinking, he was commonly requested to rinse his mouth with water to which a little honey and vinegar had been added. His looseness rather increased, and the stools were watery, black, and foetid: It was judged necessary to moderate this discharge, which seemed to sink him, by mixing a drachm of the *theriaca Andromachi* with each clyster.

21st. The same putrid symptoms remained, and a *subsultus tendinum* came on: his stools were more foetid; and so hot, that the nurse assured me she could not apply her hand to the bed-pan, immediately after they were discharged, without feeling pain on this account: The medicine and clysters were repeated.

Reflecting upon the disagreeable necessity we seemed to lie under of confining this putrid matter in the intestines, lest the evacuation should destroy the *vis vitæ* before there was time to correct its bad quality, and overcome its bad effects, by the means we were using; I considered, that, if this putrid ferment could be more immediately corrected, a stop would probably be put to the flux, which seemed to arise from, or at least to be increased by it; and the *fomes* of the disease would likewise be in a great measure removed. I thought nothing was so likely to effect this, as the introduction of fixed air into the alimentary canal,

which, from the experiments of Dr. Macbride, and those you have made since his publication, appears to be the most powerful corrector of putrefaction hitherto known. I recollected what you had recommended to me as deserving to be tried in putrid diseases, I mean, the injection of this kind of air by way of clyster, and judged that in the present case such a method was clearly indicated.

The next morning I mentioned my reflections to Dr. Hird and Dr. Crowther, who kindly attended this young gentleman at my request, and proposed the following method of treatment, which, with their approbation, was immediately entered upon. We first gave him five grains of ipecacoanha. to evacuate in the most easy manner part of the putrid *colliquies*: he was then allowed to drink freely of brisk orange-wine, which contained a good deal of fixed air, yet had not lost its sweetness: the tincture of bark was continued as before; and the water, which he drank along with it, was impregnated with fixed air from the atmosphere of a large vat of fermenting wort, in the manner I had learned from you: instead of the astringent, air alone was injected, collected from a fermenting mixture of chalk and oil of vitriol: he drank a bottle of orange-wine in the course of this day, but refused any other liquor except water and his medicine: two bladders full of air were thrown up in the afternoon.

23d. His stools were less frequent; their heat likewise and peculiar *fætor* were considerably diminished: his muttering was much abated, and the *subfultus tendinum* had left him. Finding that part of the air was rejected when given with a bladder in
the

the usual way, I contrived a method of injecting it which was not so liable to this inconvenience. I took the flexible tube of that instrument which is used for throwing up the fume of tobacco, and tied a small bladder to the end of it that is connected with the box made for receiving the tobacco, which I had previously taken off from the tube: I then put some bits of chalk into a six ounce phial until it was half filled; upon these I poured such a quantity of oil of vitriol as I thought capable of saturating the chalk, and immediately tied the bladder, which I had fixed to the tube, round the neck of the phial: the clyster pipe, which was fastened to the other end of the tube, was introduced into the *anus* before the oil of vitriol was poured upon the chalk. By this method the air passed gradually into the intestines as it was generated; the rejection of it was in a great measure prevented; and the inconvenience of keeping the patient uncovered during the operation was avoided.

24th, He was so much better, that there seemed to be no necessity for repeating the clysters: the other means were continued. The window of his room was now kept shut.

25th, All the symptoms of putrescency had left him; his tongue and teeth were clean; there remained no unnatural blackness or *factor* in his stools, which had now regained their proper consistence; his dozing and muttering were gone off; and the disagreeable odour of his breath and perspiration was no longer perceived. He took nourishment to-day, with pleasure; and, in the afternoon, sat up an hour in his chair.

His

His fever, however, did not immediately leave him; but this we attributed to his having caught cold from being incautiously uncovered, when the window was open, and the weather extremely severe; for a cough, which had troubled him in some degree from the beginning, increased, and he became likewise very hoarse for several days, his pulse, at the same time, growing quicker: but these complaints also went off, and he recovered, without any return of the bad symptoms above-mentioned.

I am, Reverend Sir,

Your obliged humble servant,

W^m Hey.

P. S.

October 29, 1772.

Fevers of the putrid kind have been so rare in this town, and in its neighbourhood, since the commencement of the present year, that I have not had an opportunity of trying again the effects of fixed air, given by way of clyster, in any case exactly similar to Mr. Lightbowne's. I have twice given water saturated with fixed air in a fever of the putrescent kind, and it agreed very well with the patients. To one of them the ærial clysters were administered, on account of a looseness, which attended the fever, though the stools were not black, nor remarkably hot or fetid.

These

These clysters did not remove the looseness, though there was often a greater interval than usual betwixt the evacuations, after the injection of them. The patient never complained of any uneasy distention of the belly from the air thrown up, which, indeed, is not to be wondered at, considering how readily this kind of air is absorbed by aqueous and other fluids, for which sufficient time was given, by the gradual manner of injecting it. Both those patients recovered, though the use of fixed air did not produce a crisis before the period on which such fevers usually terminate. They had neither of them the opportunity of drinking such wine as Mr. Lightbowne took after the use of fixed air was entered upon; and this, probably, was some disadvantage to them.

I find the methods of procuring fixed air, and impregnating water with it, which you have published, are preferable to those I made use of in Mr. Lightbowne's case.

The flexible tube used for conveying the fume of tobacco into the intestines, I find to be a very convenient instrument in this case, by the method before-mentioned (only adding water to the chalk, before the oil of vitriol is instilled, as you direct): the injection of air may be continued at pleasure, without any other inconvenience to the patient, than what may arise from his continuing in one position during the operation, which scarcely deserves to be mentioned, or from the continuance of the clyster-pipe within the anus, which is but trifling, if it be not shaken much, or pushed against the rectum.

When I said in my letter, that fixed air appeared to be the greatest corrector of putrefaction hitherto

known, your philosophical researches had not then made you acquainted with that most remarkably antiseptic property of nitrous air. Since you favoured me with a view of some astonishing proof of this, I have conceived hopes, that this kind of air may likewise be applied medicinally to great advantage.

W. II.

A CORRECTION.

Upon re-examining Dr. Hales's account of his experiments to measure the diminution of air by respiration (*Statistical Essays*, Vol. I. p. 238, 4th edition), I find an error of the press, of 1 for 10; so that the diminution of air by respiration, though very various, is, I believe, always considerably less than by putrefaction, or several other causes of diminution. But though I have mentioned this diminution as equal to several others, nothing material depends upon it: the quality of the air thus diminished being, in all respects, the same, notwithstanding the cause of increase (which, as I have observed, in this and other cases, co-operates with the cause of diminution) be greater than I had supposed.

I did not endeavour to measure the quantity of the diminution of air by respiration, as I did that by other causes; because I imagined that it had been done sufficiently by others, and especially by Dr. Hales.

Received November 29, 1771.

XX. *An Essay on the periodical Appearing and Disappearing of certain Birds, at different Times of the Year. In a Letter from the Honourable Daines Barrington, Vice-Pref. R. S. to William Watson, M. D. F. R. S.*

DEAR SIR,

Read April 2, 9, 30,
and May 14, 1772.

AS I know, from some conversation we have had on this head, that you consider the migration of birds as a very interesting point in natural history, I send you the following reflections on this subject as they have occurred to me upon looking into most of the ornithologists who have written on this question.

It will be first necessary in the present, as in all other disputes, to define the terms on which the controversy arises. I therefore premise that I mean by the word Migration, a periodical passage by a whole species of birds across a considerable extent of sea.

I do not mean therefore to deny that a bird, or birds, may possibly fly now and then from Dover to

Calais, from Gibraltar to Tangier, or any other such narrow strait, as the opposite coasts are clearly within the bird's ken, and the passage is no more adventurous than across a large fresh water lake.

I as little mean to deny that there may be a periodical flitting of certain birds from one part of a continent to another: the Royston Crow, and Rock Ouzel, furnish instances of such a regular migration.

What I mean chiefly to contend therefore is, that it seems to be highly improbable, birds should, at certain seasons, traverse large tracts of sea, or rather ocean, without leaving any of the same species behind, but the sick or wounded.

As this litigated point can only receive a satisfactory decision from very accurate observations, all preceding naturalists, from Aristotle to Ray, have spoken with much doubt concerning it.

Soon after the appearance of Mons. Adanson's voyage to Senegal, however, Mr. Collinson first, in the *Philosophical Transactions* *, and after him the most eminent ornithologists of Europe, seem to have considered this traveller's having caught four European Swallows on the 6th of October, not far from the African coast, as a decisive proof, that the common swallows, when they disappear in Europe, make for Africa during the winter, and return again to us in the spring.

It is therefore highly incumbent upon me, who profess that I am by no means satisfied with the account, given by Mons. Adanson of these European

* Part II. 1760, p. 459, & seq.

swallows, to enter into a very minute discussion of what may, or may not, be inferred from his observation according to his own narrative.

I shall first however consider the general arguments, from which it is supposed that birds of passage periodically traverse oceans, which indeed may be almost reduced to this single one, *viz.* we see certain birds in particular seasons, and afterwards we see them not; from which data it is at once inferred, that the cause of their disappearance is, that they have crossed large tracts of sea.

The obvious answer to this is, that no well-attested instances can be produced of such a migration, as I shall endeavour to shew hereafter; but besides this convincing negative proof, there are not others wanting.

Those who send birds periodically across the sea, being pressed with the very obvious answer I have before suggested, have recourse to two suppositions; by which they would account for their not being observed by seamen during their passage.

The first is, that they rise so high in the air that they become invisible*; but unfortunately the rising to this extraordinary height, or the falling from it, is equally destitute of any ocular proof, as the birds being seen during their passage.

I have indeed conversed with some people, who conceive they have lost sight of birds by their perpendicular flight; I must own, however, that I have

* It is well known that some ornithologists have even supposed that they leave our atmosphere for that of the Moon. See *Harl. Misc.* Vol. II. p. 561.

always supposed them to be short-sighted, as I never lost the sight of a bird myself, but from its horizontal distance, and I doubt much whether any bird was ever seen to rise to a greater height than perhaps twice that of St. Paul's cross*.

There seems to be but one method indeed, by which the height of a bird in the air may be estimated; which is, by comparing its apparent size with its known one, when very near us; and it need not be said that method of calculating must depend entirely upon the sight of the observer, who, if he happens not to see objects well at a distance, will very soon suppose the bird to be lost in the clouds.

There is also another objection to the hypothesis of birds passing seas at such an extraordinary height, arising from the known rarefaction of the air, which may possibly be inconvenient for respiration, as well as flight; and if this was not really the case, one should suppose that birds would frequently rise to such uncommon elevations, when they had no occasion to traverse oceans.

* Wild geese fly at the greatest height of any bird I ever happened to attend to; and from comparing them with rooks, which I have frequently looked at, when perched on the cross of St. Paul's, I cannot think that a wild-geese was ever diminished, to my sight at least, more than he would be at twice the height of St. Paul's, or perhaps 300 yards. Mr. Hunter, F. R. S. informs me, that the bird which hath appeared to him as the highest flier, is a small eagle on the confines of Spain and Portugal, which frequents high rocks. Mr. Hunter hath first seen this species of eagle from the bottom of a mountain, and followed it to the top, when the bird hath risen so high as to appear less than he did from the bottom. Mr. Hunter however adds, that he could still hear the cry, and distinguish the bird.

The Scotch Ptarmigan frequents the highest ground of any British bird, and he takes but very short flights.

But it is also urged by some, that the reason why seamen do not regularly see the migration of birds, is because they choose the night, and not the day, for the passage*.

Now though it may be allowed, that possibly birds may cross from the coast of Holland to the Eastern coast of England (for example) during a long night, yet it must be dark nearly as long as it is within the Arctic circle to afford time for a bird to pass from the Line to many parts of Europe, which *Mons. de Buffon* calculates, may be done in about eight or nine days†.

If the passage happened in half the nights of the year, which have the benefit of moonlight, the birds would be discovered by the sailors almost as well as in the day time; to which I must add that several supposed birds of passage (the Fieldfare in particular) always call when on their flight, so that the seamen must be deaf as well as blind, if such flocks of birds escape their notice.

Other objections however remain to this hypothesis of a passage during the night.

* *Mr. Catesby* supposes that they may thus pass in the night time, to avoid birds of prey. *Phil. Trans. Abr. Vol. II. p. 887.* But are not owls then stirring?

On the other hand, if they migrate in the day time, kites, hawks, and other birds of prey, must be very bad sportsmen not to attend (like Arabs) these large and periodical caravans.

† In the preface to the first volume of his lately published *Ornithology*, p. 32.

Most birds not only sleep during the night, but are as much incapacitated from distinguishing objects well as we are, in the absence of the sun: it is therefore inconceivable that they should choose owl-light for such a distant journey.

Besides this, the Eastern coast of England, to which birds of passage must necessarily first come from the continent, hath many light-houses upon it; they would therefore, in a dark night, immediately make for such an object, and destroy themselves by flying with violence against it, as is well known to every bat-fowler.

Having endeavoured to answer these two suppositions, by which it is contended that birds of passage may escape observation in their flight; I shall now consider all the instances I have been able to meet with of any birds being actually seen whilst they were crossing any extent of sea, though I might give a very short refutation to them, by insisting, that if this was ever experienced, it must happen as constantly in a sea, which is much navigated, as the return of the seasons.

I cannot do better than to follow these according to chronological order.

The first in point of time is that which is cited by Willoughby*, from Bellon, whose words are thus translated, “ When we sailed from Rhodes to
“ Alexandria, many quails flying from the North
“ towards the South, were taken in our ship, whence
“ I am persuaded that they shift places; for formerly, when I sailed out of the Isle of Zant to
“ Morea, or Negropont, in the spring, I had ob-

* B. II. c. xi. §. 8.

“ served

“ served quails flying the contrary way to N. and S.
 “ that they might abide there all summer, at which
 “ time also a great many were taken in the ship.”

Let us now consider what is to be inferred from this citation.

In the first place, Bellon does not particularize the longitude and latitude of that part of the Mediterranean, which he was then crossing; and in his course from Rhodes to Alexandria, both the islands of Scarpanto and Crete could be at no great distance: these quails therefore were probably flitting from one island of the Mediterranean* to another.

The same observation may be made with regard to the quails which he saw between Zant and Negropont, as the whole passage is crouded with islands, they therefore might be passing from island to island, or headland to headland, which might very probably lye East and West, so as to occasion the birds flying in a different direction, from which they passed the ship before.

I have therefore no objection to this proof of migration, if it is only insisted upon to shew that a quail shifts its station at certain seasons of the year; but cannot admit that it is fair from hence to argue that these birds periodically cross large tracts of sea.

Bellon himself states, that when the birds settled upon the ship, they were taken by the first person who chose to catch them, and therefore they must have been unequal to the short flight which they were attempting.

* One of the Mediterranean islands is supposed to have obtained its ancient name of Ortygia from the numbers of quails.

It is very true that quails have been often pitched upon as instances of birds that migrate across seas, because they are scarcely ever seen in winter: it is well known, however, to every sportsman, that this bird never flies 300 yards at a time, and the tail being so short, it is highly improbable they should be equal to a passage of any length.

We find therefore, that quails, which are commonly supposed to leave our island in the winter, in reality retire to the sea coasts, and pick up their food amongst the sea weeds *.

I have happened lately to see a specimen of a particular species of quail, which is described by Dr. Shaw†, and is distinguished from the other kinds by wanting the hind-claw.

Dr. Shaw also states that it is a bird of passage. Now if quails really migrate from the coast of Barbary to Italy, as is commonly supposed, whence can it have arisen that this remarkable species hath escaped the notice of Aldrovandus, Olina, and the other Italian ornithologists?

When I had just finished what I have here said with regard to the migration of quails, I have had an opportunity of seeing the second volume of Mons. de Buffon's ornithology‡; where, under this article, he contends that this bird leaves Europe in the winter.

It is incumbent upon me, therefore, either to own I am convinced by what this most ingenious and able naturalist hath urged, or to give my reasons why I

* See Br. Zool. Vol. II. p. 210. 2d Ed. octavo.

† Phys. Obs. on the kingdom of Algiers, ch. 2.

‡ See p. 459, & seq.

still continue to dissent from the opinion he maintains.

Though M. de Buffon hath discussed this point very much at large, yet I find only the following facts or arguments to be new.

He first cites the Memoirs of the Academy of Sciences *, for an account given by M. Godeheu of quails coming to the island of Malta in the month of May, and leaving it in September.

The first answer to this observation is, that the island of Malta is not only near to the coast of Africa, but to several of the Mediterranean islands; it therefore amounts to no more than the sitting I have before taken notice of †.

Monf. de Buffon supposes that a quail only quits one latitude for another, in order to meet with a perpetual crop on the ground.

Now can it be supposed that there is that difference between the harvest on the coast of Africa, and that of the small quantity of grain which grows on the rocky island of Malta, that it becomes inconvenient to the bird to stay in Africa as soon as May sets in; and necessary, on the other hand, to continue in Malta from May till September.

Monf. de Buffon then supposes that quails make their passage in the night, as well as conceives them to be of a remarkably warm temperature ‡, and says

* Tom. III. p. 91 and 92.

† Both Monf. de Godeheu and M. de Buffon seem to conceive that the quail should fly in the same direction as the wind blows; but birds on the wing from point to point, which are at a considerable distance, fly against the wind, as their plumage is otherwise ruffled.

‡ As this is given for a reason why the African quails migrate Northward: Q. what is to become of the Icelandic quails during the summer?

that "*chaud comme une caille*," is in every one's mouth *.

Now in the first place their migration during the night, is contrary to Belon's account, which M. de Buffon so much relies upon, who expressly says, that the birds were caught in the day time †.

In the next place, I apprehend that "*chaud comme une caille*," alludes to the very remarkable falaciousness of this bird, and not to the constant heat of its body.

Mons. de Buffon then observes, that if quails are kept in a cage, they are remarkably impatient of confinement in the autumn and spring, whence he infers that they then want to migrate ‡; he also adds, in the same period, that this uneasiness begins an hour before the sun rises, and that it continues all the night.

This great naturalist does not state this observation as having been made by himself, and it seems upon the face of it to be a very extraordinary one.

* All birds indeed are warmer by four degrees than other animals. See some ingenious thermometrical experiments by Mr. Martin of Aberdeen, Edinb. 1771, 12mo.

† Upon looking a second time into Belon, he does not indeed state whether it was in the day or the night; but if it had happened in the latter, this traveller and ornithologist could not well have omitted such a circumstance. Besides this, he mentions in what direction the quails were flying, which he could not have discerned in the night.

‡ It may also arise from this bird's being of so quarrelsome a disposition, and consequently most likely to fight with its fellow prisoners when they are all in greatest vigour after moulting, and on the return of the spring.

M. de Buffon allows that they will fight for a grain of millet, and adds, "*car parmi les animaux il faut un sujet reel pour se battre*." M. de Buffon hath never been in a cockpit.

No

No one (at least with us) ever keeps quails in a cage except the poulterers, who always sell them as fast as they are fat, and consequently can give no account of what happens to them during so long an imprisonment as this observation necessarily implies.

No such remarkable uneasiness hath ever been attended to in any other supposed bird of passage during its confinement; but, allowing the fact to be as M. de Buffon states, he himself supplies us with the real cause of this impatience.

He asserts, that quails constantly moult twice * a year, *viz.* at the close both of summer and winter; whence it follows, that the bird, in autumn and the spring, must be in full vigour upon its recovery from this periodical illness: it can therefore as little brook confinement, as the physician's patient upon the return of health after illness.

Thus much I have thought it necessary to say, in answer to M. de Buffon, who "dum errat, docet," who scarcely ever argues ill but when he is misinformed as to facts, and who often, from strength of understanding, disbelieves such intelligence as might impose upon a naturalist of less acuteness and penetration.

* I have often heard that certain birds moult twice a year, some of which I have kept myself without their changing their feathers more than once.

I should suppose that this notion arises from some birds not moulting regularly in the autumn every year; and when the change takes place in the following spring, they very commonly die: I can scarcely think that many of them are equal to two illnesses of so long a continuance, which are constantly to return within twelvemonths.

I should therefore rather account for the extraordinary briskness of a quail in autumn and the spring, from its recovery after moulting in the former, and from the known effects of the spring as to most animals in the latter.

The next instance of a bird being caught at any distance from land, is in Sir Hans Sloane's voyage to Jamaica, who says, that a lark was taken in the ship 40 leagues from the shore: this therefore was certainly an unfortunate bird, forced out to sea by a strong wind in flying from headland to headland, as no one supposes the skylark to be a bird of passage.

The same answer may be given to a yellow-hammer's settling upon Hasselquist's ship in the entrance of the Mediterranean, with this difference, that either the European or African coast must have been much nearer than 40 leagues*.

The next fact to be considered is what is mentioned in a letter of Mr. Peter Collinson's, printed in the Philosophical Transactions†.

He there says, "That Sir Charles Wager had frequently informed him, that in one of his voyages home in the spring as he came into soundings in our channel, that a great flock of swallows almost covered his rigging, that they were nearly spent and famished, and were only feathers and bones; but being recruited by a night's rest, they took their flight in the morning."

The first answer to this is, that if these were birds which had crossed large tracts of sea in their periodical migrations, the same accident must happen eternally, both in the spring and autumn, which is not however pretended by any one.

In the next place, the swallows are stated to be spent both by famine and fatigue; and how were they to procure any flies or other sustenance on the

* See Hasselquist's Travels, in princ.

† 1760. Part II. p. 461.

rigging of the admiral's ship, though they might indeed rest themselves?

Sir Charles, however, expressly informs us, that he was in the channel, and within soundings: these birds, therefore (like Bellon's quails) were only passing probably from headland to headland; and being forced out by a strong wind, were obliged to settle upon the first ship they saw, or otherwise must have dropped into the sea, which I make no doubt happens to many unfortunate birds under the same circumstances.

As the birds which thus settled upon Sir Charles Wager's rigging were swallows, it very naturally brings me now to consider the celebrated observation of Mons. Adanson, under all its circumstances, as it hath been so much relied upon, and by naturalists of so great eminence.

Mons. Adanson is a very ingenious writer, and the publick is much indebted to him for many of the remarks which he made whilst he resided in Senegal.

I may, however, I think, presume to say, that he had not before his voyage made ornithology his particular study; proofs of which are not wanting in other parts of his work, which do not relate to swallows.

For example, he supposes, that the Canary birds which are bred in Europe are white, and that they become so by our climate's being more cold than that of Africa.

“ J'ai remarqué que le serin qui devient tout blanc
 “ en France, est à l'eneriffe d'un gris presque aussi
 “ foncé que celui de la linotte; ce changement de
 “ couleur provient vraisemblablement de la froidure
 “ de notre climat *.”

* Voyage au Senegal, p. 13..

Mr. Adanson in this passage seems to have deduced two fallacious inferences from having seen a few white Canary birds in France, which he afterwards compares with those of Teneriff, and supposes the change of colour to arise merely from alteration of climate: it is known, however, almost to every one, that there is an infinite variety in the plumage of the European Canary birds, which, as in poultry, arises from their being pampered with so much food, as well as confinement*.

Monsr. Adanson, in another part of his voyage†, describes a Roller, which he supposes to migrate sometimes to the Southern parts of Europe.

This circumstance shews that he could not have looked much into books of natural history, because the principal synonym of this bird is *garrulus Argentoratensis*‡; and Linnæus informs us that it is found even in Sweden||.

* In the same passage, he compares the colour of the African Canary bird to that of the European linnet, and says it is *d'un gris presque aussi foncé*, whereas the European linnet is well known to be brown, and not grey. The linnet affords a very decisive proof that the change of plumage does not arise from the difference of climate, but the two causes I have assigned. The cock bird, whilst at liberty, hath a red breast: yet if it is either bred up in a cage from the nest, or is caught with its red plumage, and afterwards moults in the house, it never recovers the red feathers.

That most able naturalist, Monsr. de Buffon, from having seen some cock linnets which had thus moulted off, or perhaps some hen linnets (which have not a red breast) considers them as a distinct species, and compares their breeding together in an aviary, to that of the Canary bird and goldfinch. Ornith. p. xxiii.

† P. 16. ‡ Or of Strasburgh.

|| Faun. Suec. 94.

The strong characteristic mark of this bird, is the outermost feathers of the tail, which able naturalists describe as three fourths of an inch longer than the rest*. Mons. Adanson, however, compares their length, not with the other feathers of the tail, but with the length of the bird's body, which is by no means the natural or proper standard of comparison.

The reason of my taking notice of these more minute inaccuracies in Mons. Adanson's account of birds, arises from Mr. Collinson's relying upon his observations with regard to swallows being so absolutely decisive, because he is represented to be so able a naturalist.

I shall now state (very minutely) under what circumstances these swallows were caught, and what seems to be the true inference from his own account.

He informs us, that four swallows settled upon the ship, not 50 leagues from the coast of Senegal, on the 6th of October; that these birds were taken, and that he knew them to be the true swallow of Europe†, which he supposes were then returning to the coast of Africa.

I shall now endeavour to shew that these birds could not be European swallows; nor, if they were, could they have been on their return from Europe to Africa.

* Willoughby, p. 131. Br. Zool. Vol. II. in append.

† I have before endeavoured to shew that Mons. Adanson does not always recollect with accuracy the plumage of the most common European birds, by what he says with regard to the linnet.

The word *hirondelle*, in French, is used as a general term for the four * species of these birds, as the term *swallow* is with us.

Now the four swallows thus caught and examined by Mons. Adanson were either all of the same species, or intermixed in some other proportion.

Would not then any naturalist in stating so material a fact (as he himself supposes it to be) have particularized of what species of swallow these very interesting birds were?

Should not Mons. Adanson also have taken care to distinguish these supposed European swallows from two species of the same tribe, which bear a general resemblance to those of Europe, and are not only described, but engraved by Brisson, under the name of *Hirondelle de Senegal* & *Hirondelle de rivage du Senegal* †?

Though Mons. Adanson was above a year on this part of the African coast, paid so much attention to swallows, and was so immediately acquainted with the different species on the first inspection, yet he seems never to have discovered that there were such African swallows as are thus described and engraved by Brisson, though he must have seen them daily.

Mons. Adanson however concludes his account of the supposed European swallow, whilst it continues on the coast of Senegal, by a circumstance which

* Viz. the swallow and *chayw*, the martin, the sand martin, and the swift: I omit the goatfucker, because this bird, though usually classed as a species of swallow by ornithologists, is not so considered by others.

† See Brisson, Tom. II. pl. xiv.

seems to prove to demonstration of what species the four swallows caught in the ship really were.

He says that they roost on the sand either by themselves, or at most only in pairs, and that they frequent the coast much more than the inland parts *.

These swallows therefore, if they came from Europe, must have immediately changed at once their known habits: and is it not consequently most clear that they were of that species which Brisson describes under the name of *Hirondelle de rivage du Senegal*?

But though it should be admitted, notwithstanding what I have insisted upon, from Mons. Adanson's own account, that these were really swallows of the same kind with those of Europe; yet I must still contend that they could not possibly have been on their return from Europe to Africa, because the high road for a bird from the most Western point of Europe to Senegal, is along the N. West coast of Africa, which projects greatly to the Westward of any part of Europe.

What then could be the inducement to these four swallows to fly 50 leagues to the Westward of the coast of Senegal, so much out of the proper direction?

It seems to me therefore, very clear, that these swallows (whether of the European kind or not) were sitting from the cape de Verde islands to the

* Voyage au Senegal, p. 67. I wish Mons. Adanson had also informed us whether these swallows had the same notes with those of Europe, which is a very material circumstance in the natural history of birds, though little attended to by most ornithologists.

coast of Africa, to which short flight, however, they were unequal, and were obliged from fatigue to fall into the sailors hands.

Mons. Adanson likewise mentions * that the ship's company caught a Roller on the 26th of April, which he supposes was on its passage to Europe, though he was then within sight of the coast of Senegal: this bird, however, must be admitted not to have had sufficient strength to reach the first stage of this round-about journey, and was therefore probably forced out to sea by a strong wind, in passing from head-land to head-land.

But I must not dismiss what hath been observed with regard to the swallows seen by Mons. Adanson at Senegal, without endeavouring also to answer what M. de Buffon hath not only inferred from it, but hath endeavoured to confirm by an actual experiment †.

M. de Buffon, from the many instances of swallows being found torpid even under water, very readily admits, that all the birds of this genus do not migrate, but only that species which was seen by Mons. Adanson in Africa, and which he generally refers to as the chimney swallow ‡; but from the outset, seems

* Voyage au Senegal, p. 15.

† See the two prefatory discourses to his sixteenth volume of natural history.

‡ So little do naturalists know of this very common bird, that I believe it hath never yet been observed by any writer, that the male swallow hath only the long slender feathers in the tail, which are considered as its most distinguishing marks. I venture to make this remark upon having seen the difference in two swallows which are in Mr. Tunstall's collection, F. R. S. as also in two others, which have lately been presented to the Museum

to shew that he hath himself confounded this species with the martin.

“ Prenons un seul oiseau, par exemple, l'hirondelle, celle que tout le monde connoit, qui paroît au printems, dispaeroit en automne, &c fait son nid avec de la terre contre les fenestres, ou dans les cheminees.” p. 23.

It is very clear that the design in this period is to specify a particular bird in such a manner that no doubt could remain with any one about the species referred to; and from other passages which follow, it is as clear that *Monf. de Buffon* means to allude to the swallow *κατ' ἐξοχην*.

Though this was certainly the intention of this most ingenious naturalist, it is to me very evident that the martin, and not the swallow, was in his contemplation, because he first speaks of the bird's building against windows, before he mentions chimneys, and therefore supposes that either place is indifferent; which is not the case, because the swallow seldom builds on the sides of windows, or the martin in chimneys.

There are perhaps three or four martins to one swallow in all parts; and from their being the more common bird of the two, as well as from the circumstance of their building at the corner of windows (and consequently being eternally in our sight), nine-

of the Royal Society, by the directors of the Hudson's Bay company.

These long feathers would be very inconvenient to the hen during incubation; and they are likewise confined to the cock *widow-bird*, as, from their more extraordinary length, they would be still more so.

teen out of twenty, when they speak of a swallow, really mean a martin *.

I only take notice of this supposed inaccuracy in *Mons. de Buffon*, because, if that able naturalist does not speak of the different sorts of swallows with that precision which is necessary upon such an occasion, why should he rely so intirely upon the impossibility of *Mons. Adanson's* being mistaken?

I shall now state the experiment of *Mons. de Buffon*, to prove that the swallow is not torpid in the winter, and must therefore migrate to the coast of Senegal †.

He shut up some swallows (*hirondelles*) in an ice house, which were there confined “plus ou moins de temps;” and the consequence was, that those which remained there the longest died, nor could they be revived by exposing them to the sun; and, that those “qui n'avoient souffert le froid de la glaciere que pendant peu de tems” were very lively when permitted to make their escape.

* In the same manner the genetical name in other languages, for this tribe of birds, always means the martin, and not the swallow.

Thus *Anacreon* complains of the *χελιδων* for waking him by its twittering.

Now if it be considered that there was only the kitchen chimney in a Grecian house, it must have been the martin which built under the eaves of the window, that was troublesome to *Anacreon*, and not the swallow.

Ovid also speaking of the nest of the *hirundo*, says,

—— luteum sub trabe figit opus.

by which he necessarily alludes to the martin, and not the swallow.

† *Plan de l'ouvrage*, p. 15.

Mons.

Monf. de Buffon does not, in this account of his experiment, state the time during which the birds were confined; but as the trial must have been made in France, the swallows which he procured could not be expected to be torpid either in an ice-house* or any other place, because the season for their being in that state was not yet arrived.

I cannot also agree with M. de Buffon that those birds which were shut up the longest time died through cold, as he supposes, but for want of food, as he neither supplied them with any flies, nor, if he had, could the swallows have caught them in the dark: a very short fast kills these tender animals, which are feeding every instant when on the wing.

It therefore seems not to follow from this, or any other experiment, that swallows must necessarily migrate (as Monf. de Buffon supposes) to the coast of Senegal.

* The very name of an ice-house almost strikes one with a chill; I placed, however, a thermometer in one near Hyde Park Corner, on the 23d of November, where it continued 48 hours, and the mercury then stood at $43\frac{1}{2}$ by Fahrenheit's scale.

This is therefore a degree of cold which swallows sometimes experience whilst they continue in some parts of Europe, without any apparent inconvenience; and it should seem that the cold vapours which may arise from the included ice, sink the thermometer only 7 or 8 degrees, as the temperature in approved cellars is commonly from 50 or 51 throughout the year.

Sir William Hamilton informs me, that he hath frequently seen swallows in the winter between Naples and Puzzuoli, when the weather was warm; as does Mr. Hunter, F. R. S. that he hath observed them during the same season, on the confines of Spain and Portugal. It should seem from this, that very mild and warm weather for any continuance always wakes these birds from their state of torpidity.

Swallows.

Swallows are seen during the summer, in every part of Europe from Lapland to the Southern coast of Spain; nor is Europe vastly inferior in point of size to Africa.

If swallows therefore retreat to Africa in the winter, should not they be dispersed over the whole Continent of Africa, just as they are over every part of Europe?

But this most certainly is not so: Dr. Shaw, who was a very good naturalist and attended much to the birds in the neighbourhood of Algiers (as appears by his account of that country), makes no mention of any such circumstance, nor have we heard of it from any other traveller*.

It must be admitted indeed, that Herodotus speaking of a part of upper Egypt (which he had never seen) says, that kites and swallows never leave it†; this, however, totally differs from Mons. Adanson's account, who informs us that they disappear in Senegal on the approach of summer.

It seems to follow therefore, from this silence in others, that swallows cannot be accommodated for their winter residence in any part of that vast continent, but in the neighbourhood of Senegal.

But this is not the whole objection to such an hypothesis.

* It may also be observed here, that credit is in some measure given to M. Adanson's eyesight, against that of all the English, French, Dutch, Portuguese, and Danes, who have been settled not far from Senegal for above a century, many of which have spent the greatest part of their lives there, and whose notice, swallows seen during the winter, must have probably attracted.

† *Ἰσθμὸς δὲ καὶ χελιδόνες δὲ αἷος τοῖς καὶ ἀπολείπεται.* Euterpe, p. 98. ed. Gale.

If the swallows of Europe, when they disappear in those parts, retreat to the coast of Senegal, what necessarily follows with regard to a Lapland swallow?

I will suppose such a bird to have arrived safely at his winter quarters upon the approach of that season in Lapland; but he must then, according both to *Monf. Adanson's* and *de Buffon's* account, return to Lapland in the spring, or at least some other swallow from Senegal fill his place*.

Such a bird immediately upon its arrival on the Southern coast of Spain would find the climate and food which it desired to attain, and all proper conveniences for its nest: what then is to be its inducement for quitting all these accommodations which it meets with in such profusion, and pushing on immediately over so many degrees of European continent to Lapland, where both martin and swallow can procure so few eaves of houses to build upon? What also is to be the inducement to these birds, when they have arrived at that part of the Norwegian coast which is opposite to the *Ferroe* islands, to cross degrees of sea, in order

* *Mr. Stephens, A. S. S.* informs me, that there was a nest of martins for twenty years together in the hall of his house in *Somersetshire* (near *Bath*); nor could the old birds procure food either for themselves, or their young, till the door was opened in the morning.

Can it be supposed that the same birds or their descendants could have so long fixed upon so very inconvenient a spot, to which they constantly returned from the coast of Africa, neglecting so many others, which they must have always passed by? Does it not also afford a most strong presumption, that they were torpid during winter in the neighbourhood of this old hall?

to build in such small spots of land, where there are still fewer houses?

The next fact I have happened to meet with of a bird's being seen at a considerable distance from the shore, is in Mr. Forster's lately published translation of Kalm's account of N. America*.

We are there informed that a bird (which Kalm calls a swallow) was seen near the ship on the 2d of September, and, as he supposes, 20 degrees from the continent of America†.

It appears however, by what he before states in his journal, that the ship was not above 5 degrees from the island of Sable.

Besides, if it is contended that this was an European swallow on its passage across the Atlantic on the 2d of September, it is too early even for a swift, to have been on its migration, which disappears with us sooner than the three other species of European swallows‡.

Only two more instances have occurred of birds being seen in *open* sea that have been described

* Vol. I. p. 24.

† It may not be improper here to observe, that in all instances of birds being seen at sea any great distance from the coast, it is not improbable that they may have before settled on some other vessel, or perhaps on a piece of floating wreck.

By accidents of this sort, even butterflies have sometimes been caught by the sailors at 40 leagues distance from any land. See Mons. l'Abbé Courte de la Blanchadiere's Voyage to Brazil, Paris, 1759, 21mo. p. 169.

‡ The bird mentioned by Kalm was probably an American swallow, forced out to sea by some accidental storm: there are several species of them and they seem to bear a general affinity to those of Europe.

with any sort of precision, which I shall just state, as I would not decline giving the best answer I am able to every argument and fact which may be relied upon, by those who contend that birds periodically migrate across oceans.

On the 30th of March, 1751, Osbeck, in his voyage from Sweden to China *, met with a single house swallow near the Canary Islands, which was so tired that it was caught by the sailors: Osbeck also states, that though it had been fine weather for several preceding days, the bird was as wet as if it had just emerged from the bottom of the sea.

If this instance proves any thing, it is the submersion and not the migration of swallows so generally believed in all the northern parts of Europe. It would swell this Letter to a most unreasonable size, to touch only upon this litigated point; and I shall, for the present, suppress what hath happened to occur to me on this controverted question †.

* See the lately published translation of this voyage.

† I will, however, mention one most decisive fact on this head.

Mr. Stephens, A. S. S. informs me, that, when he was fourteen years of age, a pond of his father's (who was vicar of Shrivenham in Berkshire) was cleaned, during the month of February; that he picked up himself a cluster of three or four swallows (or martins), which were caked together in the mud, and that he carried them into the kitchen, on which they soon afterwards flew about the room, in the presence of his father, mother, and others. Mr. Stephens also told me, that his father (who was a naturalist) observed at the time, he had read of similar instances in the northern writers. This fact is also confirmed to me by the Reverend Dr. Pye, who was then at school in Shrivenham, as also by a very sensible land-surveyor, who now lives in the village.

Osbeck afterwards, in the course of his voyage, mentions, that a swallow (indefinitely) followed the ship, near Java, on the 24th of July, and another on the 14th of August, in the Chinese sea, as he terms it.

After what I have observed before with regard to other instances of the same sort, I need scarcely say that this naturalist does not state of what species these swallows were; and that, from the latitudes in which they were seen, they must have been some of the Asiatic kinds.

I cannot, however, dismiss this article of the swallow, without adding some general reasons, which seem to prove the great improbability of this or any other bird's periodically migrating over wide tracts of sea; and I the rather do it in this place, because

There are several reasons why swallows should not be frequently thus found; ponds are seldom cleaned in the winter, as it is such cold work for the labourers; and the same instinct which prompts the bird thus to conceal itself, instructs it to choose such a place of security, that common accidents will not discover it.

But the strongest reason for such accounts not being more numerous, is, that facts of this sort are so little attended to; for though I was born within half a mile of this pond, and have always had much curiosity with regard to such facts, yet I never heard a syllable about this very material and interesting account, till very lately.

To this fact I must also add, that swallows may be constantly taken in the month of October, during the dark nights, whilst they sit on the willows in the Thames, and that one may almost instantaneously fill a large sack with them, because at this time they will not stir from the twigs, when you lay your hands upon them. This looks very much like their beginning to be torpid before they hide themselves under the water.

A man near Brentford says, that he hath caught them in this state in the eyt opposite to that town, even so late as November.

the

the swallow is commonly pitched upon as the most notorious instance of such a regular passage.

This seems to arise first from its being seen in such numbers during the summer, from its appearing almost always on the wing, and from its feeding in that position; from which two latter circumstances it is supposed to be the best adapted for such distant migrations.

And first, let us consider, from the few facts or reasons we have to argue from, what length of flight either a swallow or any other bird is probably equal to.

A swallow, it is true, seems to be always on the wing; but I have frequently attended, as much as I could, on a particular one; and it hath appeared to me, that the bird commonly returned to its nest in eight or ten minutes: as for extent of flight, I believe I may venture to say, that these birds are seldom a quarter of mile from their mate or young ones; they feed whilst on the wing, and are perpetually turning short round to catch the insects, who endeavour to elude them as a hare does a greyhound.

It therefore seems to me, that swallows are by no means equal to long flights, from their practice during their summer residence with us.

I have long attended to the flight of birds; and it hath always appeared to me, that they are never on the wing for amusement (as we walk or ride), but merely in search of food.

The only bird which I have ever observed to fly without any particular point of direction, is the rook: these birds will, when the wind is high,

“ Ride in the whirlwind, and enjoy the storm.”

They never fly, however, at this time, from point to point, but only tumble in the air, merely for their diversion.

It seems, therefore, that birds are by no means calculated for flights across oceans, for which they have no previous practice: and they are, in fact, always so fatigued, that, when they meet a ship at sea, they forget all apprehensions, and deliver themselves up to the sailors.

Let us now consider another objection to the migration of the swallow, which *Mons. de Buffon* supposes may cross the Atlantic to the Line in eight days*; and this not only from the want of rest, but of food, during the passage.

A swallow, indeed, feeds on the wing: but where is it to find any insects, whilst it is flying over a wide expanse of sea? This bird, therefore, if it ever attempted so adventurous a passage, would soon feel a want of food, and return again to land, where it had met with a constant supply from minute to minute.

I am aware it may be here objected, that the swallow leaves us on the approach of winter, when soon no flying insects can be procured: but I shall hereafter endeavour to shew, that these birds are then torpid, and, consequently, can want no such food.

Another objection remains to the hypothesis of migration, which is, that birds, when flying from

* *Discours sur la nature des oiseaux*, p. 32.

point to point, endeavour always to have the wind against them *, as is periodically experienced by the London bird-catchers, in March and October, when they lay their nets for singing birds †.

The reason, probably, for birds thus flying against the wind is, that their plumage may not be ruffled, which indeed I have before had occasion to mention.

Let us suppose, then, a swallow to be equal to a passage across the Atlantic in other respects; how is the bird to be insured of the wind's continuing for days in the same quarter; or how is he to depend upon its continuing to blow against his flight with moderation? for who can suppose that a swallow can make his way to the point of direction, when buffeted by a storm blowing in the teeth of his intended passage ‡?

Lastly, can it be conceived that these, or any other birds, can be impelled by a providential instinct, regularly to attempt what seems to be attended with such insuperable difficulties, and what most frequently leads to certain destruction?

But it will still be objected, that as swallows regularly appear and disappear at certain seasons, it is incumbent upon those who deny their migration, to

* Kalm, in his voyage to America, makes the same observation, with regard to flying fish, and Valentine says, that if the wind does not continue to blow against the bird of paradise, it immediately drops to the ground.

† These birds, as it should seem, are then in motion; because, at those seasons, the ground is plowed either for the winter or lent corn.

‡ I have myself attended to swallows during a high wind, and have observed that they fly only in sheltered places, whilst they almost touch the surface of the ground.

shew what becomes of them in Europe during our winter.

Though it might be answered, that it is not necessary, those who endeavour to shew the impossibility of another system or hypothesis, should from thence be obliged to set up one of their own; yet I shall, without any difficulty, say, that I at least am convinced swallows (and perhaps some other birds) are torpid during the winter.

I have not, I must own, myself ever seen them in this state; but, having heard instances of their being thus found, from others of undoubted veracity, I have not scarcely the least doubt with regard to this point.

It is, indeed, rather difficult to conceive why some ornithologists continue to withhold their assents to such a cloud of witnesses, except that it perhaps contradicts a favourite hypothesis which they have already maintained.

Why is it more extraordinary that swallows should be torpid during the winter, than that bats are found in this state, and so many insects, which are the food of swallows?

But it may be said, that as the swallows have crowded the air during the summer, in every part of Europe since the creation, and as regularly disappear in winter, why have not the instances of their being found in a torpid state been more frequent?

To this it may be answered, that though our globe may have been formed so many centuries, yet the inhabitants of it have scarcely paid any attention to the study of natural history, but within these late years.

As for the ancient Greeks and Romans, their dress prevented their being so much in the fields as we are; or, if they heard of a rather extraordinary bird in their neighbourhood, they had not a gun to shoot it: the only method of attaining real knowledge in natural history, depends almost entirely upon the having frequent opportunities of thus killing animals, and examining them when dead.

If they did not stir much in their own country, much less did they think of travelling into distant regions; want of bills of exchange, and of that curiosity which arises from our being thoroughly acquainted with what is near us at home, probably occasioned this; to which may also be added, the want of a variety of languages: scarcely any Greek seems to have known more than his own tongue, nor Roman more than two*.

Aristotle, indeed, began something like a system of natural history, and Pliny put down, in his common place-book, many an idle story; but, before the invention of printing, copies of their works could not be so generally dispersed; as to occasion much attention to what might be interesting facts for the natural historian.

In the sixteenth century, Gesner, Belon, and Aldrovandus, published some materials, which might be of use to future naturalists; but, in the seventeenth, Ray and Willoughby first treated this extensive branch of study, with that clearness of method,

* It need be scarcely here mentioned also, that their navigation was confined to the Mediterranean, from the compass not having been then discovered.

perspicuity of description, and accuracy of observation, as hath not, perhaps, been since exceeded.

The works of these great naturalists were soon dispersed over Europe, and the merit of them acknowledged; but it so happened, that Sir Isaac Newton's amazing discoveries in natural philosophy making their appearance about the same time, engaged entirely the attention of the learned.

In process of time, all controversy was silenced by the demonstration of the Newtonian system; and then the philosophical part of Europe naturally turned their thoughts to other branches of science.

Since this period, therefore, and not before, natural history hath been studied in most countries of Europe; and consequently, the finding swallows in a state of torpidity, or on the coast of Senegal, during the winter, begins to be an interesting fact, which is communicated to the world by the person who observes it.

To this I may add, that the common labourers, who have the best chance of finding torpid birds, have scarcely any of them a doubt with regard to this point; and consequently, when they happen to see them in this state, make no mention of it to others; because they consider the discovery as neither uncommon or interesting to any one.

Molyneux, therefore, in the Philosophical Transactions *, informs us, that this is the general belief of the common people of Ireland, with regard to land-rails; and I have myself received the same answer from a person who, in December, found swallows torpid in the stump of an old tree.

* Phil. Trans. abr. Vol. II. p. 853.

Another reason why the instances of torpid swallows may not be expected so frequently, is, that the instinct of secreting themselves at the proper season of the year, likewise suggests to them, it's being necessary to hide themselves in such holes and caverns, as may not only elude the search of man, but of every other animal which might prey upon them; it is not therefore by any common accident that they are ever discovered in a state of torpidity.

Since the study of natural history, however, hath become more general, proofs of this fact are frequently communicated, as may appear in the *British Zoology* *.

That it may not be said, however, I do not refer to any instance which deserves credit, if properly sifted, I beg leave to cite the letter from Mr. Achard to Mr. Collinson, printed in the *Philosophical Transactions* †, from whence it seems to be a most irrefragable fact, that swallows ‡ are annually discovered in a torpid state on the banks of the Rhine. I shall also refer to Dr. Birch's *History of the Royal Society* ||, where it is stated, that the celebrated Harvey dissected

* See Vol. II. p. 250. *Brit. Zool.* ill. p. 13, 14. As also Mr. Pennant's *Tour in Scotland*, p. 199.

† 1763, p. 101.

‡ "Swallows or martins," are Mr. Achard's words, which I the rather mention, because Mr. Collinson complains that the species is not specified.

Mr. Collinson himself had endeavoured to prove, that sand martins are not torpid, *Phil. Trans.* 1760, p. 109. and concludes his letter, by supposing that all the swallow tribe migrates, therefore the swift is the only species remaining; for his friend Mr. Achard shews to demonstration, that swallows or martins are torpid; he does not, indeed, precisely state which of them.

|| Vol. IV. p. 537.

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some, which were found in the winter, under water, and in which he could not observe any circulation of the blood *.

Assuming it, therefore, from these facts, that swallows have been found in such a state, I would ask the partisans of migration, whether any instance can be produced where the same animal is calculated for a state of torpidity and, at the same time of the year, for a flight across oceans ?

But it may be urged, possibly, that if swallows are torpid when they disappear, the same thing should happen with regard to other birds, which are not seen in particular parts of the year.

To this I answer, that this is by no means a necessary inference: if, for example, it should be insisted that other birds besides the cuckow are equally careless with regard to their eggs, it would be immediately allowed that the argument arising from

* As the swallows were found in the winter, they must have been in a state of torpidity, as otherwise the animals must have been putrid.

I shall likewise here refer to *Phil. Transf. abr.* Vol. V. p. 33. where Mr. Derham says, that he heard a swift squeak in an hole of his house on the 17th of April; but that, the weather being cold, it did not stir abroad for several days.

This seems to be a strong instance of a bird's first waking from a state of torpidity, but resuming its sleep on the weather being severe.

I shall close the proofs on this head (which I could much enlarge) by the dignified testimony of Sigismond, King of Poland, who affirmed on his oath, to the cardinal Commendon, that he had frequently seen swallows, which were found at the bottom of lakes. See the life of cardinal Commendon, p. 211. Paris, 1671. 4to.

such supposed analogy could by no means be relied upon *.

It is possible, however, that some other birds, which are conceived to migrate, may be really torpid as well as swallows; and if it be asked why they are not sometimes also seen in such a state during the winter, the answer seems to be, that perhaps there may be a thousand swallows to any other sort of bird, and that they commonly are found torpid in clusters.

* I here suppose the common notion about the cuckow to be true; because both learned and ignorant seem equally to agree in the fact.

During the present summer, however, a girl brought a full feathered young cuckow to a gentleman's house, where I happened to be, who said, that it had been for several days before fed by another bird of equal size with itself; which therefore could not be a hedge-sparrow, or other small bird, but the parent cuckow.

I have also lately been favoured, by Mr. Pennant, with the following extract from a manuscript of Derham's on instinct.

“ The Rev. Mr. Stafford was walking in Glossop-dale in the Peak of Derbyshire, and saw a cuckow rise from its nest, which was on the stump of a tree, that had been some time felled, so as much to resemble the colour of the bird. In this nest were two young cuckows, one of which he fastened to the ground, by means of a peg and line, and very frequently, for many days, beheld the old cuckow feed these her young ones.”

It is not impossible, therefore, that this most general opinion will turn out like the supposed effects of the venom of the tarantula; and, indeed, it is difficult to conceive how so small a bird as a hedge-sparrow can feed a cuckow: it is also remarkable, that the witnesses often vary about the species of small bird thus employed.

It is possible, however, that the cuckow (though it may not hatch its young) may feed them, when grown too large for the foster parent.

If a single bird of any other kind happens to be seen in the winter, without motion or apparent warmth, it is immediately conceived that it died by some common accident.

I shall, however, without any reserve, say, that I rather conceive the notion which prevails with regard to the migration of many birds, may most commonly arise from the want of observation, and ready knowledge of them, when they are seen on the wing, even by professed ornithologists.

It is an old saying, that “ a bird in the hand is worth two in the bush;” and this holds equally with regard to their being distinguished, when those even who study natural history, have but a transient sight of the animal*.

If, therefore, a bird, which is supposed to migrate in the winter, passes almost under the nose of a Linnaean, he pays but little attention to it, because he cannot examine the beak, by which he is to class the bird. Thus I conceive, that the supposing a nightingale to be a bird of passage arises from not readily distinguishing it, when seen in a hedge, or on the wing†.

This bird is known to the ear of every one, by its most striking and capital notes, but to the eye of very

* An ingenious friend of mine makes always a very proper distinction between what he calls in-door and out-door naturalists.

Thomas Willisel, who assisted Ray and Willughby much with regard to the natural history of the animals of this island, never stirred any where without his gun and fishing-tackle.

† No two birds fly in the same manner, if their motions are accurately attended to.

few indeed; because the plumage is dull, nor is there any thing peculiar in its make.

The nightingale sings perhaps for two months *, and then is never heard again till the return of the spring, when it is supposed to migrate to us from the continent, with redstarts, and several other birds.

That it cannot really do so, seems highly probable, from the following reasons.

This bird is scarcely ever seen to fly above twenty yards, but creeps at the bottom of the hedges, in search of maggots, and other insects, which are found in the ground.

If the swallow is not supplied with any food during its passage across oceans, much less can the nightingale be so accommodated; and I have great reason to believe, from the death of birds in a cage, which have had nothing to eat for twenty-four hours, that these delicate and tender animals cannot support a longer fast, though using no exercise at all.

To this I may also add, that those birds which feed on insects are vastly more feeble than those whose bills can crack seed, and consequently, less capable of bearing any extraordinary hardships or fatigue.

But other proofs are not wanting, that this bird cannot migrate from England.

* Whilst it sings even, the bird can seldom be distinguished, because it is then almost perpetually in hedges, when the foliage is thickest, upon the first burst of the spring, and when no insects can as yet have destroyed considerable parts of the leaves.

Nightingales are very common in Denmark, Sweden, and Russia *, as also in every other part of Europe, as well as Asia, if the Arabic name is properly translated.

Now, if it is supposed that many of these birds which are observed in the southern parts of England, cross the German sea, from the opposite coast of the continent; why does not the same instinct drive those of Denmark to Scotland, where no such bird was ever seen or heard †?

But these are not all the difficulties which attend the hypothesis of migration; nightingales are agreed to be scarcely ever observed to the westward of Dorsetshire, or in the principality of Wales ‡, much less in Ireland.

I have also been informed, that these birds are not uncommon in Worcestershire, whereas they are excessively rare (if found at all) in the neighbouring county of Hereford.

Whence, therefore, can it arise, that this bird should at one time be equal to the crossing of seas, and at other times not travel a mile or two into an adjacent county? Does it not afford, on the other hand, a strong proof, that the bird really continues

* See Dr. Birch's History of the Royal Society, Vol. III. p. 189. *Linnei Fauna Suecica*. and *Biographia Britannica*, art. FLITCHER; where it is said, that they have in Russia a greater variety of notes than elsewhere.

† Sir Robert Sibbald, indeed, conceives the nightingale to be a bird of North Britain; but, if I can depend upon many concurrent testimonies, no such bird is ever seen or heard so far northward at present, nor could I ever trace them in that direction further than Durham.

‡ I have, however, frequently seen the nightingale's congener (and supposed fellow-traveller) the redstart in Wales.

on the same spot during the whole year, but happens not to be attended to, from the reasons I have before suggested?

I am therefore convinced, that if I was ever to live in the country during the winter, I should see nightingales, because I should be looking after them, and I am accordingly informed, by a person who is well acquainted with this bird, that he hath frequently observed them during this season *.

If it be asked, why the nightingales are all this time mute? the answer is, that the same silence is experienced in many other birds, and this very muteness is, in part the cause why the bird is not attended to in winter.

I must now ask those who contend for the migration of a nightingale, what is to be its inducement for crossing from the continent to us? a swallow, indeed, may want flies in winter, if it stays in England; but a nightingale is just as well supplied with insects on the continent, as it can be with us after its passage †. I must also ask, in what other part of

* I find they have also been seen in France during the winter. See a treatise, intitled, *Aëdologue*, Paris 1751. p. 23.

† I have omitted the mention of a more minute proof, that this bird cannot migrate from the continent, from the having kept them for some years in a cage, and having been very attentive to their song.

Kircher (in his *Musurgia*) hath given us the nightingale's notes in musical characters, from which it appears that the song of a German nightingale differs very materially from that of an English one: now, if there was a communication by migration between the continent and England, the song of these birds would not so materially differ, as I may, perhaps, shew, by some experiments I have made, in relation to the notes of birds.

I have before mentioned, that Mr. Fletcher, who was ambassador from England to Russia in the time of Queen Elizabeth,
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the world this bird is seen during the winter? must it migrate to Senegal with the swallow?

I am persuaded likewise, that the cuckow never migrates from this island any more than the nightingale: this bird is either probably torpid in the winter, or otherwise is mistaken for one of the smaller kind of hawks*; which it would be likewise in the spring, was it not for its very particular note at that time, and which only lasts during courtship, as it does with the quail.

If there is fine weather in February, this bird sometimes makes this sort of call to its mate, whilst it is supposed to continue still on the continent.

An instance is mentioned by Mr. Bradley †, of not only a single cuckow, but several, which were heard in Lincolnshire, during the month of February; and that able naturalist Mr. Pennant informs me, another was heard near Hetcham in Shropshire, on the 4th of February in the present year ‡.

observed that the song of the Russian nightingale differed from that of the English.

* Mr. Hunter, F. R. S. informs me, that he hath seen cuckows in the island of Belleisle during the winter, which is not situated so much to the southward, as to make it improbable that they may equally continue with us.

† Works of Nature, p. 77.

‡ Mr. Pennant received this account from Mr. Plimly, of Longnor in Shropshire.

Thus likewise Mr. Edwards informs us, that the sea fowls near the Needles, which are commonly supposed to migrate in winter, appear upon the weather's being very mild. Essays, p. 197.

It is amazing how much the being interested to discover particular objects contributes to our readily distinguishing them.

I remember the being much surprized that a grey-headed game-keeper always saw the partridge on the ground before they rose, when I could not do the same. He told me, however, that the reason was, I lived in a time when the shooter had no occasion to give himself that trouble.

He then further explained himself, by saying, that when he was young, no one ever thought of aiming at a bird when on the wing, and consequently they were obliged to see the game before it was sprung. He added, that from this necessity he could not only distinguish partridges, but snipes and woodcocks, on the ground.

Another instance of the same kind, is the great readiness with which a person, who is fond of coursing, finds a hare sitting in her form : those, however, who are not interested about such sport, can scarcely see the hare, when it is under their nose, and pointed out to them.

But more apparent objects escape our notice, when we are not interested about them.

Ask any one, who hath not a botanical turn, what he hath seen in passing through a rich meadow, at the time it is most enamelled with plants in flower ; and he will tell you, that he hath observed nothing but grass and daisies. If most gardeners even are in like manner asked whether the flowers of a bean grow on every side of the stalk, they will suppose that they do,

whereas they, in reality, are only to be found on one side.

The mouths of flounders are often turned different ways, which one would think could not well escape the observation of the London fishmongers; yet, upon asking several of them whether they had attended to this particular, I found they had not, till I shewed them the proof in their own shops.

A fishmonger, however, knows immediately whether a fish is in good eating order or not, on the first inspection; because this is a circumstance which interests him.

I shall, however, by no means suppress two arguments in favour of migration, which seem to require the fullest answer that can be given to them.

The first is, that there are certain birds, which appear during the winter, but disappear during the summer; and it may be asked, where such birds can be supposed to breed, if they do not migrate from this island.

These birds are in number four, viz. the snipe, woodcock, redwing, and fieldfare.

As for the snipe, I have a very short answer to give to the objection, as far as it relates to this bird; because it constantly breeds in the fens of Lincolnshire, Wolmar forest, and Bodmyn downs; it is therefore highly probable, that it does the same in almost every county of England.

I mustown, however, that, till within these few years, I conceived the nest of a snipe was as rarely seen in England, as that of a woodcock or fieldfare; and that able ornithologist Mr. Edwards supposes this to be

be the fact, in the late publication of his ingenious *Essays on Natural History* *.

Woodcocks likewise are known to build in some parts of England every year; but, as the instances are commonly those of a single nest, I would by no means pretend to draw the same proof against the summer migration of this bird, as in the former case of the snipe.

I will most readily admit, that these accidental facts are rather to be accounted for, perhaps, from the whimsy or silliness of a few birds, which occasions their laying their eggs in a place where they are easily discovered, and contrary to what is usual with the bulk of the species.

I remember to have seen a duck's nest once on the top of a pollard willow, near the decoy in St. James's Park; it would not be, however, fair to infer from such an instance, that all ducks would pitch upon the same very improper situation for a nest, upon which it is difficult to conceive how a web-footed bird could settle.

Some silly birds likewise now and then choose a place for building, which cannot escape the observation of either man or beast, as he passes by.

I therefore suppose that the few proofs of woodcocks nests having been found in England, arise either from one or other of these two causes, and all which they seem to prove is, that our climate in summer is not absolutely improper for them.

It is to be observed, however, that Mr. Catesby considers such instances as of equal force against the

migration of the woodcock, as of the snipe *. Willughby also says, that Mr. Jessop saw young woodcocks sold at Sheffield (which rather implies a certain number being brought to market), and that others had observed the same elsewhere †.

We are, indeed, informed by Scopoli ‡, that they breed constantly in Carniola, which is considerably to the southward of any part of England: our country is therefore certainly not too hot for them.

Woodcocks appear and disappear almost exactly about the same time in every part of Europe, and perhaps Africa ||: heat and cold, therefore, seem not to have any operation whatsoever with regard to the supposed migration of this bird.

But it may be said, what signifies proving the probability of woodcocks breeding in England, if it is not a known fact that they do so?

To this it should seem there are several answers, as it is equally incumbent upon those who contend for migration, to shew that these birds were ever seen on such passage.

Another answer is, ask ninety-nine people out of a hundred, whether snipes ever make a nest in England; and they will immediately say, that they do not; so little are facts or observations of this sort attended to.

But I shall now endeavour to give some other reasons why woodcocks may not only continue with us

* Phil. Trans. abr. Vol. II. p. 889.

† B. iii. c. 1.

‡ Ornith. Leipsig, 1769.

§ Shaw's Trav. Phys. Obs. ch. ii.

during the summer, but also breed in large tracts of wood or bog, without being observed.

In the other parts of Europe, all birds almost are considered as game, or, at least, are eaten as wholesome food, Ray therefore mentions, that hawks and owls are sold by the poulterers at Rome; every sort of small bird also is equally the foreign fowler's object *.

An Englishman does not consider, on the other hand, perhaps twelve kinds of birds worthy his attention, or expence of powder, none of which are ever shot in our woods during the summer, nor are birds then disturbed by felling either coppice or timber.

But it will be said, why are not woodcocks sometimes seen, however, as they may be supposed to leave their cover in search of food?

To this I answer, that woodcocks sleep always in the daytime, whilst with us in the winter, and feed only during the night †. Whenever a woodcock, therefore, is flushed, he is roused from his sleep by the spaniel or sportsman, and then takes wing, because there are no leaves on the trees to conceal the bird.

Whoever hath looked attentively at a woodcock's eye, must see that, from the appearance of it, the

*. In one of Boccaccio's Novels, a lover, who lives at Florence, dresses a falcon for the dinner of his mistress. *Giorna a V. Novel. IX.*

† Almost all the wild fowl of the duck kind also sleep in the daytime, and feed at night.

fight must be more calculated to distinguish objects by night than by day *.

The fact therefore is notorious to those who cut glades in their woods, and fix nets for catching these birds, that they never stir but as it begins to be dark, after which they return again by day-break, when their fight even then is so indifferent, that they strike against the net, and thus become entangled.

No one with us ever thinks of fixing or attending such nets in summer for woodcocks, because it is not then supposed that there is any such bird in the island; if they tried this experiment, however, I must own that I believe they would have sport †.

Mr. Reinhold Forster, F. R. S. who is an able naturalist, informs me, that the fowlers in the neighbourhood of Dantzick kill many woodcocks about St. John's day (or Midsummer), in the following man-

* I conceive also, it is from the eyes looking so dull, that this bird is generally considered as being so foolish: hence the Africans call the woodcock *hammar el bulgel*, or the partridge's ass. Shaw's Phys. Obs. ch. ii.

† I would ask those who will probably laugh at the very idea of such sport (which I do not, however, absolutely insure), whether, if I was to send them to any part of the British coast to catch the true anchovy, or tunny fish, they would not suppose equally that it was a fool's errand.

Notwithstanding, however, this incredulity, I can produce the authority of both Ray (Syn. Pisc. p. 107.) and Mr. Pennant (Brit. Zool. III. p. 34. 36.), that the true anchovy is caught in the sea not far from Chester, and the tunny fish on the coast of Argyleshire, together with the herrings, where they are called *mackrel sturs*.

Is it not amazing, however, that a fish of such a size as the tunny should never have been heard of, even by the Scotch naturalist Sir Robert Sibbald?

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ner, and that they continue to do so till the month of August.

They wait on the side of some of the extensive woods in that neighbourhood, before day-break, for the return of the woodcock from his feeding in the night-time, and always depend upon having a very good chance of thus shooting many of them.

The Dantzickers, however, might be employed the whole summer near these woods in the day-time, without ever seeing such a bird; and it seems therefore not improbable, that it arises from our not waiting for them at twilight or day-break, that they are never observed by Englishmen in the summer. If this bird should, however, be seen in the night, it is immediately supposed to be an owl, which a woodcock does not differ much from in its flight.

To these reasons for woodcocks not being observed, it may be added, that the bird is believed to be absolutely mute, and consequently, never discovers itself by its call.

If it be still contended, that the nest or young must sometimes be stumbled upon, though in the centre of extensive woods, or large bogs, the fiskin (or aberdavine *) is a much more extraordinary instance of concealing its nest and young.

The plumage of this bird is rather bright than otherwise; and the song, though not very pleasing, yet is very audible, both which circumstances should discover it at all times; yet Kramer † informs us, that, though immense numbers breed annually on

* Brit. Zool. p. 309.

† Elenchus Animalium per Austriam, p. 261. Viennæ, 1756.

the banks of the Derwent, no one ever observed the male.

The bird is rather uncommon in England; so that it is odd, when the nest was ever found within the verge of the flood, it may be considered as rather an unfair challenge.

There is another bird, however, called a redpoll*, which is taken in numbers during the Michaelmas and March flights by the London bird-catchers, whose nest, I believe, was never discovered in England, though I have seen them in pairs during the summer, both in the mountainous parts of Wales and highlands of Scotland †.

But I shall now mention another proof that woodcocks breed in England.

The Reverend Mr. White, of Selborn, who is not only a well-read naturalist, but an active sportsman, informs me, that he hath frequently killed woodcocks in March, which, upon being opened, had the rudiments of eggs in them, and that it is usual at that time to flush them in pairs. Wallughby also observes the same ‡.

This bird, therefore, certainly pairs before its supposed migration; and can it be conceived that this strict union (which birds in a wild state so faithfully adhere to) §, should take place before they

* Brit. Zool. p. 312.

† The elegant little bird is very common in Hudson's Bay, where it feeds chiefly on the larch-trees; which being more common in the northern than southern parts of Great Britain, may account for the bird's being more often seen northward.

‡ B. III. c. i.

§ It is believed that no male-bird was ever seen in a wild state, notwithstanding M. de Buffon suspects many an intrigue
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traverse oceans, and when they cannot as yet have pitched upon a proper place for concealing their nest and nestlings?

Let us examine if this intercourse before migration takes place in other birds, which are supposed to cross wide extents of sea: and a quail affords such proof.

I have been present when these birds have been caught in the spring, which always turn out to be males, and are enticed to the nets by the call of the hen; quails therefore pair after they appear in England.

But I shall now consider the other two instances of birds which are seen with us in the winter, and are not observed in the summer; I mean, the fieldfare and redwing.

And first, let us examine, where these birds are actually known to breed: the northern naturalists say, in Sweden; Klein, in the neighbourhood of Dantzick, which is only in lat. $54^{\circ} 30'$ *; and Wülgby, in Bohemia.

in the recesses of the woods (Hist. Nat. des. Oiseaux; tom. I.) such irregular intercourse is only observed in cages and aviaries, where birds are not only confined, but pampered with food.

* See Klein, de Avibus Erraticis, p. 178. Klein, however, cites Zornius, who lived in the same part of Germany, and who asserts that the *turdus iliacus* (or redwing) leaves those parts in the spring. The circumstance therefore of the redwing's breeding in numbers (*per multitudines*) had escaped the notice of Zornius, though he hath written a dissertation on this question.

Is it at all surprizing, after this, that such discoveries, if made at all, should not be commonly heard of?

As they therefore build their nests in more Southern parts of Europe, there is certainly no natural impossibility of their doing so with us, though, I must own, I never yet heard but of one instance, which was a fieldfare's nest found near Paddington*.

I cannot, however, but think it is only from want of observation, that more of such nests have not been discovered, which are only looked after by very young children; and the chief object is the eggs, or nestlings, not the bird which lays them†.

The plumage therefore and flight of the fieldfare or redwing being neither of them very remarkable, it is not at all improbable they may remain in summer, without being attended to; and particularly the redwing, which scarcely differs at all in appearance from other thrushes. Thus the cough is by no means peculiar to Cornwall, as is commonly supposed, but is mistaken for the jackdaw, or rook.

But it may be said, that these birds fly in flocks during the winter, and if they remain here during the summer, we should see them equally congregate.

I have not before referred to Klein, who hath written a very able treatise, in which he argues against the possibility of migration in birds; because, though I should be very happy to support my poor opinion by his authority, yet I thought it right neither to repeat his facts, or arguments.

* See also Harl. Misc. Vol. II. p. 561.

† Many birds also build in places of such difficult access, that boys cannot climb to; birds nesting is confined almost entirely to hedges, and low shrubs.

This circumstance, however, is by no means peculiar to the fieldfare and redwing; most of the hard-billed singing birds do the same in winter, but separate in summer, as it is indeed necessary all birds should during the time of breeding.

I shall now consider another argument in favour of migration, which I do not know hath been ever insisted upon by those writers who have contended for it, and which at first appearance seems to carry great weight with it.

There are certain birds, which are supposed to visit this island only at distant intervals of years; the Bohemian chatterer and cross-bill * (for example) once perhaps in twenty.

The fact is not disputed, that such birds are not commonly observed in particular spots from year to year; but this may arise from two causes, either a partial migration within the verge of our island, or perhaps more frequently from want of a ready knowledge of birds on the wing, when they happen to be seen indeed, but cannot be examined.

I never have disputed such a partial migration; and indeed I have received a most irrefragable proof of such a flitting, from the Rev. Mr. White of Selborn in Hampshire, whose accurate observations I have before had occasion to argue from.

* This bird changes the colour of its plumage at different seasons of the year, which is sometimes red.

The first account we have of their being seen, is in the Ph. Tr. abr. Vol. V. p. 33. where Mr. Edward Lhwyd suspects them to be Virginia nightingales, from their feathers being red, and had no difficulty of at once supposing that they had crossed the Atlantic.

The rock (or ring-ouzel) hath always hitherto been considered as frequenting only the more mountainous parts of this island: Mr. White, however, informs me that there is a regular migration of these birds, which flock in numbers, and regularly visit the neighbourhood of Selborn, in Hampshire*.

I therefore have little doubt but that they equally appear in others of our Southern counties; though it escapes common observation, as they bear a sort of general resemblance to the black-bird, at least to the den of that species.

I own also, that I always conceived the Bohemian chatterer was not observed in Great Britain but at very distant intervals of years, and then perhaps only a single bird, whereas Dr. Ramsay (professor of natural history at Edinburgh) informs Mr. Pennant, that flocks of these birds appear constantly every year in the neighbourhood of that city†.

As for cross-bills, they are seen more and more in different parts of England, since there have been so many plantations of firs: this bird is remarkably fond of the seeds of these trees, and therefore changes its place to those parts where it can procure the greatest plenty of such food‡.

* See also Br. Zool. III. p. 56.

† These birds are said to be particularly fond of the berries of the mountain-ash, which is an uncommon tree in the Southern parts of Great Britain, but by no means so in the North.

‡ This bird should also, for the same reason, be found from year to year in the cyder counties, if it was true (as is commonly supposed) that he is particularly fond of the kernels of

This flitting therefore by no means amounts to a total and periodical migration over seas, but is no more than what is experienced with regard to several birds.

For example, the British Zoology informs us *, that, at an average, 4000 dozen of larks are sent up from the neighbourhood of Dunstable, to supply the London markets; nor do I hear, upon inquiry, that there is any complaint of the numbers decreasing from year to year, notwithstanding this great consumption.

I should not suppose that 50 dozen of skylarks are caught in any other county of England; and it should therefore seem that the larks from the more adjacent parts crowd in to supply the vacuum occasioned by the London Epicures, which may be the cause possibly of a partial migration throughout the whole island.

I begin now to approach to something like a conclusion of this (I fear) tedious dissertation: I think, however, that I should not omit what appears to me at least as a demonstration, that one bird, which is commonly supposed to migrate across seas, cannot possibly do so.

apples, which it is conceived he can instantly extract with his very singular bill.

Mr. Tunstall, F. R. S. however, at my desire, once placed an apple in the cage of a cross-bill, which he had kept for some time in his very valuable and capital collection of live birds: upon examining the apple a fortnight afterwards, it remained untouched.

* P. 235.

A landrail *, when put up by the shooter, never flies 100 yards; its motion is excessively slow, whilst the legs hang down like those of the water fowls which have not web feet, and which are known never to take longer flights.

This bird is not very common with us in England, but is excessively so in Ireland, where they are called corn-creaks.

Now those who contend that the landrail, because it happens to disappear in winter, must migrate across oceans, are reduced to the following dilemma.

They must first either suppose that it reaches Ireland periodically from America; which is impossible, not only because the passage of the Atlantic includes so many degrees of longitude, but because there is no such bird in that part of the globe.

If the landrail therefore migrates from the continent of Europe to Ireland, which it must otherwise do, the necessary consequence is, that many must pass over England in their way Westward to Ireland; and why do not more of these birds continue with us, but, on the contrary, immediately proceed across the St. George's channel?

Whence should it arise also, if they pass over this island periodically in the spring and autumn, that they are never observed in such passage, as I have already stated their rate in flying to be excessively slow; to which I may add, that I never saw them rise to the height of twenty yards from the ground, nor indeed exceed the pitch of a quail.

* Br. Zool. p. 387.

I have now submitted the best answers that have occurred, not only to the general arguments for the migration of birds across oceans, but also to the particular facts, which are relied upon as actual proofs of such a regular and periodical passage.

Though I may be possibly mistaken in many of the conjectures I have made, yet I think I cannot be confuted but by new facts, and to such fresh evidence, properly authenticated, I shall most readily give up every point, which I have from present conviction been contending for.

I may then perhaps also flatter myself, that the having expressed my doubts with regard to the proofs hitherto relied upon, in support of migration, may have contributed to such new, and more accurate observations.

It is to be wished, however, that these more convincing and decisive facts may be received from islanders (the more distant from any land the better*) and not from the inhabitants of a continent; as it does not seem to be a fair inference, because certain birds leave certain spots at particular times, that they therefore migrate across a wide extent of sea.

For example, storks disappear in Holland during the winter, and they have not a very wide tract of sea between them and England; yet this bird never frequents our coasts.

* I would particularly propose the islands of Madera and St. Helena; to these, I would also add the island of Ascension (had it any inhabitants), as likewise Juan Fernandez, for the Pacifick ocean.

The stork, however, may be truly considered a bird of passage, by the inhabitants of those parts of Europe (wherever situated) to which it may be supposed to resort during the winter, and where it is not seen during the summer.

I am, dear Sir,

Your most faithful,

humble servant,

Daines Barrington.

SINCE I sent to you my very long letter on the migration of birds, I have had an opportunity of examining the "Planches Enluminées," which are said to be published under M. de Buffon's inspection, and which seem to afford a demonstration of M. Adanson's inaccuracy in supposing either the roller, or swallows, which he caught in his ship, near the coast of Senegal, to be the same with those of Europe.

In the 8th of these plates, there is a coloured figure of a bird, called le rolhier d'Angola, which agrees exactly with M. Adanson's description *; but he trusted too much to his memory, when he pronounced it to be the same with the *Garrulus Argentoratenfis* of Willughby, and therefore supposed it to be on its passage to Europe.

This bird hath, indeed, in many respects, a very strong resemblance to the common roller of Europe, which is represented also in the *Planches Enluminées*, plate 486; but it differs most materially in the length of the two exterior feathers of the tail, as well as in the colour of the neck, which in the African roller is of a most bright green, and in the European of rather a dull blue.

In the 310th plate, there is likewise a coloured representation of the "Hirondelle a ventre roux du Senegal," which specimen was possibly furnished by Monsr. Adanson himself.

* Voyage au Senegal, p. 15. There is also another African bird, represented in the "Planches Enluminées," which might very easily, on a hasty inspection, be mistaken for the *Garrulus Argentoratenfis*, viz. the Guepier a longue queue du Senegal. Pl. Enl. p. 314.

The roller of Angola is also engraved by Brisson, T. ii. pl. 7.

It very much resembles the European swallow, but the tail differs, as the fork (in the Senegal specimen) taper from the top of the two exterior feathers to the bottom, at three regular divisions, whereas in the European they are nearly of the same width throughout.

The convincing proof, however, that the "Hirondelle à ventre rouge du Senegal" differs from our chimney swallow is, that the rump is entirely covered with a bright orange or chestnut, which in the European swallow "is of a very lovely but dark purplish blue colour *."

I having lately looked into Aristotle's Natural History, with regard to the cuckow, I take this opportunity also of enlarging on the doubts I have thrown out, in relation to the prevailing notion of this bird's nestlings being hatched and fed by foster parents.

I find that this most general opinion takes its rise from what is said by this father of natural history, in his ninth book, and twenty-ninth chapter.

Aristotle there asserts, that the cuckow does not build a nest itself, but makes use most commonly of those of the wood-pigeon, hedge-sparrow, lark, (which he adds are on the ground) as well as that of the *χλωρίς* †, which is in trees.

Now, if we take the whole of this account together, it is certainly not to be depended upon; for the wood-pigeon ‡ and hedge-sparrow do not build upon the ground, and no one ever pretended to have

* See Willughby, p. 312.

† The *χλωρίς* is rendered *intola*; but, as there is no description, it is difficult to say what bird Aristotle here alludes to; Zinanni supposes it to be the greenfinch.

‡ The wood-pigeon, from its size, seems to be the only bird which is capable of hatching, or feeding, the young cuckow.

found a cuckow's egg in the nest of a lark, which, indeed, is so placed.

I have before observed, that the witnesses often vary with regard to the bird in which the cuckow's egg is deposited *; and Aristotle himself, in the seventh chapter of his sixth book, confines the foster-parents to the wood-pigeon and hedge-sparrow, but chiefly the former.

If the age † of Aristotle is considered, when he began to collect the materials for his Natural History, by the encouragement of Alexander after his conquests in India ‡, it is highly improbable he should have written from his own observations. He therefore seems to have hastily put down the accounts of the persons who brought him the different specimens from most parts of the then known world.

Inaccurate, however, and contradictory as these reports often turn out, it was the best compilation which the ancients could have recourse to; and Pliny

knew; yet, if it is recollected that this bird lives on seeds, it is probable that the cuckow, whose nourishment is insects, would either be soon starved, or incapable of digesting what was brought by the foster-parent. This objection is equally applicable to the *χλωρίς*, if it is our greenfinch.

* Thus Linnaeus supposes it (in the *Fauna Suecica*) to be the white wagtail, which bird builds in the banks of rivers, or roofs of houses, (See Zinanni, p. 51.) where it is believed no young cuckow was ever found.

† He did not leave the school of Plato till the age of thirty-eight (or, as some say, forty); after which, some years passed before he became Alexander's preceptor, who was then but fourteen: nor could he have written his Natural History, probably, till twelve years after this, as Pliny states that specimens were sent to him by Alexander, from his conquests in India. Aristotle therefore must have been nearly sixty, when he began this great work, and consequently must have described from the observations of others.

‡ Pliny, L. viii. c. 16.

therefore professes only to abridge him, in which he often does not do justice to the original.

Whatever was asserted by Aristotle, is well known to have been most implicitly believed, till the last century; and I am convinced that many of the learned in Europe would, before that time, not have credited their own eyesight against what he had delivered.

There cannot be a stronger proof that the general notion about the cuckoo arises from what is laid down by Aristotle, than the chapter which immediately follows, as it relates to the goatsucker, and states that this bird sucks the teats of that animal.

From this circumstance, the goatsucker hath obtained a similar name in most languages, though it is believed no one (who thinks at all about matters of this sort) continues to believe that this bird sucks the goat*, any more than the hedgehog does the cow.

I beg leave, however, to explain myself, that I give these additional reasons only for my doubting with regard to this most prevailing opinion; because I am truly sensible that many things happen in nature, which contradict all arguments from analogy, and I am persuaded, therefore, that the first person who gave an account of the flying fish, was not credited by any one, though the existence of this animal is not now to be disputed.

All that I mean to contend for is, that the instances of such extraordinary peculiarities in animals, should be proportionably well attested, in all the necessary circumstances.

I must own, for example, that nothing short of the following particulars will thoroughly satisfy me on this head.

* See Zinanni p. 95. who took great pains to detect this vulgar error.

The hedge-sparrow's nest must be found with the proper eggs in it, which should be destroyed by the cuckow, at the time she introduces her single egg*.

The nest should then be examined at a proper distance from day to day, during the hedge-sparrow's incubation, as also the motions of the foster parent attended to, particularly in feeding the young cuckow, till it is able to shift for itself.

As I have little doubt that the last mentioned circumstance will appear decisive to many, without the others which I have required, it may be proper to give my reasons, why I cannot consider it alone, as sufficient.

There is something in the cry of a nestling for food, which affects all kinds of birds, almost as much as that of an infant, for the same purpose, excites the compassion of every human hearer†.

I have taken four young ones from a hen skylark, and placed in their room five nestling nightingales, as well as five wrens; the greater part of which were reared by the foster parent.

It can hardly in this experiment be contended, that the skylark mistook them for her own nestlings, be-

* I could also wish that the following experiment was tried. When a hedge-sparrow hath laid all her eggs, a single one of any other bird, as large as a cuckow, might be introduced, after which if either the nest was deserted, or the egg too large to be hatched, it would afford a strong presumption against this prevailing opinion. I must here also take notice, that Mr. Hunter, F. R. S., who hath dissected hen cuckows, informs me that they are not incapacitated from hatching their eggs, as hath been supposed by some ornithologists.

† I am persuaded that a cuckow is oftener an orphan, than any other nestling, because, from the curiosity which prevails with regard to this bird, the parents are eternally shot.

cause they differed greatly, not only in number and size, but in their habits, for nightingales and wrens perch, which a skylark is almost incapable of, though, by great assiduity, she at last taught herself the proper equilibrium of the body.

I have likewise been witness of the following experiment: two robins hatched five young ones in a breeding cage, to which five others were added, and the old birds brought up the whole number, making no distinction between them.

The *Aëdologie* also mentions (which is a very sensible treatise on the nightingale *) that nestlings of all sorts may be reared in the same manner, by introducing them to a caged bird, which is supplied with the proper food.

Not only old birds, however, attend to this cry of distress from nestlings, but young ones also which are able to shift for themselves.

I have seen a chicken, not above two months old, take as much care of younger chickens, as the parent would have shewn to them which they had lost, not only by scratching to procure them food, but by covering them with her wings; and I have little doubt but that she would have done the same by young ducks.

I have likewise been witness of nestling thrushes of a later brood, being fed by a young bird which was hatched earlier, and which indeed rather over-crammed the orphans intrusted to her care; if the bird however erred in judgement, she was certainly not deficient in tenderness, which I am persuaded she would have equally extended to a nestling cuckow.

* Paris, 1751, or 1771.

Received February 13, 1772.

XXII. ΚΟΣΚΙΝΟΝ ΕΡΑΤΟΣΘΕΝΟΥΣ.

O R,

The Sieve of Eratosthencs.

*Being an account of his method of finding all
the Prime Numbers, by the Rev. Samuel
Horsley, F. R. S.*

Read May 7,
1772.

A Prime number is such a one, as hath
no intregal divisor but unity.

A number, which hath any other integral divisor,
is Composite.

Two or more numbers, which have no common
integral divisor, besides unity, are said to be Prime
with respect to one another.

Two or more numbers, which have any common
integral divisor besides unity, are said to be
Composite with respect to one another.

The distinction of numbers into Prime and
Composite, is so generally understood, that I sup-
pose it is needless to enlarge upon it.

To determine, whether several numbers proposed
be Prime or Composite *with respect to one another*,
is an easy Problem. The solution of it is given by
Euclid, in the three first propositions of the 7th

book of the Elements, and is to be found in many common treatises of Arithmetic and Algebra. But to determine, concerning any number proposed, whether it be *absolutely* Prime or Composite, is a Problem of much greater difficulty. It seems indeed incapable of a direct solution, by any general method; because the successive formation of the prime numbers doth not seem reducible to any general law. And for the same reason, no direct method hath hitherto been hit upon, for constructing a Table of all the prime numbers to any given limit. Eratosthenes, whose skill in every branch of the philosophy and literature of his times, rendered his name so famous among the Sages of the Alexandrian School, was the inventor of an indirect method, by which such a table might be constructed, and carried to a great length, in a short time, and with little labour. This extraordinary and useful invention is at present, I believe, little, or at all, known; being described only by two writers, who are seldom read, and by them but obscurely; by Nicomachus Gerasimus, a shallow writer of the 3d or 4th century, who seems to have been led into mathematical speculations, not so much by any genius for them, as by a fondness for the mysteries of the Pythagorean and Platonic philosophy; and by Boethius, whose treatise upon numbers is but an abridgment of the wretched performance of Nicomachus*. I flatter myself therefore, that a succinct account of it will not be unacceptable to this learned Society.

* There are more pieces than one of this Nicomachus extant. That which I refer to is intitled *Elementa Arithmetice*.

But before I enter expressly upon the subject, I must take the liberty to animadvert upon a certain Table, which, among other pieces ascribed to Eratosthenes, is printed at the end of the beautiful edition of Aratus published at Oxford in the year 1672, and is adorned with the title of *Κοσμικὸν Ἐρατοσθένους*. It contains all the odd numbers from 3 to 113 inclusive, distributed in little cells, all the divisors of every Composite number being placed over it, in its proper cell, and the Prime numbers are distinguished, so far as the table goes, by having no divisors placed over them. It hath probably been copied either from a Greek comment upon the Arithmetic of Nicomachus, preserved among the manuscripts of Mr. Selden in the Bodleian Library, in which, though the manuscript is now so much decayed as to be in most places illegible, I find plain vestiges of such a table *, which might be more perfect 100 years ago, when the Oxford Aratus was published; or else, from another comment, translated from a Greek manuscript into Latin, and published in that language, by Camerarius, in which a table of the very same form occurs, extending from the number 3 to 109 inclusive. It may sufficiently screen the editor of Aratus from censure, that he had these authorities to publish this table as the Sieve of Eratosthenes; especially as they are in some measure supported by passages of Nicomachus himself. But the Sieve of Eratosthenes was quite another thing.

* This manuscript seems to have contained the text of Nicomachus with Scholia in the margin. But the table evidently belongs to the Scholia, not to the text.

The Oxford editor hath annexed to his table, to explain the use of it, some detached passages, which he hath selected from the text of Nicomachus, and from a comment upon Nicomachus ascribed to Joannes Grammaticus. In these passages the difference between Prime and Composite numbers is explained, in many words indeed, but not with the greatest accuracy; and it is proposed to frame a kind of Table of all the odd numbers, from 3 to any given limit, in which the Composite numbers should be distinguished by certain marks *. The Primes would consequently be characterised, as far as the table should be carried, by being unmarked. But, upon what principles, or by what rule, such a table is to be constructed, is not at all explained. It is obvious that, in order to *mark* the Composite numbers, it is necessary to know which are such. And, without some rule to distinguish which numbers are Prime, and which are Composite, independent of any table in which they shall be distinguished by marks, it is impossible to judge, whether the table be true, as far as it goes, or to extend it, if requisite, to a further limit. Now it was the Rule by which the Prime numbers and the Composite might be distinguished, not a Table constructed we know not how, that was the invention of Eratosthenes, to which from its use, as well as from the nature of the operation, which

* Nicomachus and Joannes Grammaticus propose that these marks should be such, as should not only distinguish the composite numbers, but likewise serve to express all the divisors of every such number. It will be shewn, in a proper place, that this was no part of the original contrivance of the Sieve.

proceeds (as will be shewn) by a gradual extermination of the composite numbers from the arithmetical series 3. 5. 7. 9. 11. &c. infinitely continued, its author gave the name of the Sieve. I have thought it necessary to premise these remarks, to remove a prejudice, which I apprehend many may have conceived, as this beautiful and valuable edition of Aratus is in every ones hands, that this ill-contrived table, the useless work of some monk in a barbarous age, was the whole of the invention of the great Eratosthenes, and in justice to myself, that I might not be suspected of attempting to reap another's harvest.

I now proceed, to give a true account of this excellent invention; which, for its usefulness, as well as for its simplicity, I cannot but consider as one of the most precious remnants of Ancient Arithmetic. I shall venture to represent it according to my own ideas, not obliging myself to conform, in every particular, to the account of Nicomachus, which I am persuaded is in many circumstances erroneous. In stating the principles upon which the Operation of the Sieve was founded, he hath added observations upon certain relations of the odd numbers to one another, which are certainly his own, because they are of no importance in themselves, and are quite foreign to the purpose. Every thing of this kind I omit: and having stated what I take to have been the genuine Theory of Eratosthenes's method, cleared from the adulterations of Nicomachus, I deduce from it an operation of great simplicity, which solves the Problem in question with wonderful ease, and which,

because it is the most simple that the theory seems to afford, I scruple not to adopt as the original Operation of the Sieve, though nothing like it is to be found in Nicomachus; though, on the contrary, Nichomachus, and all his Commentators, would suggest an operation very different from it, and far more laborious. For the satisfaction of the curious and the learned, I have annexed a copy of so much of Nicomachus's treatise, as relates to this subject, with such corrections of the text, as it stands in the edition of Wiche-lius, printed at Paris ann. 1538, as the sense hath suggested to me, or I have thought proper to adopt, upon the authority of a manuscript preserved among those of Archbishop Laud, in the Bodleian Library; which, in this part, I have carefully collated. By comparing this with the account which I subjoin, every one will be able to judge how far I have done justice to the invention I have undertaken to explain.

P R O B L E M.

To find all the Prime Numbers.

The number 2 is a Prime number; but, except 2, no even number is Prime, because every even number, except 2, is divisible by 2, and is therefore Composite. Hence it follows, that all the Prime numbers, except the number 2, are included in the series of the odd numbers, in their natural order, infinitely extended; that is, in the series

3. 5. 7. 9. 11. 13. 15. 17. 19. 21. 23. 25. 27.
29. 31. 33. 35. 37. 39. 41. 43. 45. 47. 49. 51. &c.
Every

Every number which is not Prime, is a multiple of some Prime number, as Euclid hath demonstrated (Element. 7. prop. 33.) Therefore the foregoing series consists of the Prime numbers, and of multiples of the Primes. And the multiples, of every number in the series, follow at regular distances; by attending to which circumstance, all the multiples, that is, all the Composite numbers, may be easily distinguished and exterminated.

I say, the multiples of all numbers, in the foregoing series, follow at regular distances.

For between 3 and its first multiple in the series (9) two numbers intervene, which are not multiples of 3. Between 9 and the next multiple of 3 (15) two numbers likewise intervene, which are not multiples of 3. Again between 15 and the next multiple of 3 (21) two numbers intervene, which are not multiples of 3; and so on. Again, between 5 and its first multiple (15) four numbers intervene, which are not multiples of 5. And between 15 and the next multiple of 5 (25) four numbers intervene which are not multiples of 5; and so on. In like manner, between every pair of the multiples of 7, as they stand in their natural order in the series, 6 numbers intervene which, are not multiples of 7. Universally, between every two multiples of any number n , as they stand in their natural order in the series, $n-1$ numbers intervene, which are not multiples of n .

Hence may be derived an Operation for exterminating the Composite numbers, which I take to have been the Operation of the Sieve, and is as follows.

The Operation of the Sieve.

Count all the terms of the series following the number 3, by threes, and expunge every third number. Thus all the multiples of 3 are expunged. The first uncanceled number that appears in the series, after 3, is 5. Expunge the square of 5. Count all the terms of the series, which follow the square of 5, by fives, and expunge every fifth number, if not expunged before. Thus all the multiples of five are expunged, which were not at first expunged, among the multiples of 3. The next uncanceled number to 5 is 7. Expunge the square of 7. Count all the terms of the series following the square of 7, by sevens, and expunge every seventh number, if not expunged before. Thus all the multiples of 7 are expunged, which were not before expunged among the multiples of 3 or 5. The next uncanceled number which is now to be found in the series, after 7, is 11. Expunge the square of 11. Count all the terms of the series, which follow the square

3. 5. 7. 11. 13. 17. 19. 23. 25. 27. 29. 31.
 33. 35. 37. 39. 41. 43. 45. 47. 49. 51. 53. 55. 57. 59.
 61. 63. 65. 67. 69. 71. 73. 75. 77. 79. 81. 83. 85. 87.
 89. 91. 93. 95. 97. 99. 101. 103. 105. 107. 109. 111. 113.
 115. 117. 119. 121. 123. 125. 127. 129. 131. 133. 135.
 137. 139. 141. 143. 145. 147. 149. 151. 153. 155. 157.

of 11, by elevens, and expunge every eleventh number, if not expunged before. Thus all the multiples of 11 are expunged, which were not before expunged among the multiples of 3, 5, and 7. Continue these expunctions, till the first uncanceled number that appears, next to that whose multiples have been last expunged, is such, that its square is greater than the last and greatest number to which the series is extended. The numbers which then remain uncanceled are all the Prime numbers, except the number 2, which occur in the natural progression of number from 1 to the limit of the series. By the limit of the series I mean the last and greatest number to which it is thought proper to extend it.

Thus the prime numbers are found to any given limit.

Nicomachus proposes to make such marks over the Composite numbers, as should shew all the divisors of each. From this circumstance, and from the repeated intimations both of Nicomachus, and his commentator Joannes Grammaticus *, one would be led to imagine, that the Sieve of Eratosthenes was something more than its name imports, a method of lifting out the Prime numbers from the indiscriminate mass of all numbers Prime and Composite, and that, in some way or other, it exhibited all the divisors of every Composite number, and likewise shewed whether two or

* The Comment of Joannes Grammaticus is extant in manuscript in the Savilian Library at Oxford, to which I have frequent access, by the favour of the Reverend and Learned Mr. Hornsby, the Savilian Professor of Astronomy.

more Composite numbers were Prime or Composite with respect to each other. I have many reasons to think, that this was not the case. I shall as briefly as possible point out some of the chief, for the matter is not so important, as to justify my troubling the Society with a minute detail of them. First then, in the natural series of odd numbers, 3. 5. 7. &c. every number is a divisor of some succeeding number. Therefore if we are to have marks for all the different divisors of every Composite number, we must have a different mark for every odd number. Therefore we must have as many marks, or systems of marks, as numbers; and I do not see, that it would be possible, to find any more compendious marks, than the common numeral characters. This being the case, it would be impracticable to carry such a table as Nicomachus proposes, and his commentators have sketched, to a sufficient length to be of use, on account of the multiplicity of the divisors of many numbers, and the confusion which this circumstance would create*. It is hardly to be supposed, that Eratosthenes could overlook this obvious difficulty, though Nicomachus hath not attended to it. Eratosthenes therefore could not intend the construction of such a table.

In the next place, such a table not being had, Eratosthenes could not but perceive, that, the determining whether two or more numbers be Prime or Composite with respect to one another, is in all cases to be done more easily, by the direct method given by Euclid, than by

* The number 3465 hath no less than 22 different divisors.

the method of the Sieve. And he could not mean, to apply this method to a problem, to which another was better adapted.

Lastly, Eratosthenes could not mean, that the method of the Sieve should be applied to the finding of all the possible divisors of any Composite number proposed, because he could not be unacquainted with a more ready way of doing this, founded upon two obvious Theorems, which could not be unknown to him.

The Theorems I mean are these.

1st. *If two Prime numbers multiply each other, the number produced hath no divisors but the two prime factors.*

2d. *If a Prime number multiply a Composite number, and likewise multiply all the divisors of that composite severally, the numbers produced by the multiplications of these divisors will be divisors of the number produced by the first multiplication: And the number produced by the first multiplication will have no divisors, but the two factors, the divisors of the Composite factor, and the numbers made by the multiplication of these divisors by the Prime factor severally.*

The method of finding all the divisors of any Composite number, delivered by Sir Isaac Newton in the *Arithmetica Universalis*, and by Mr. Maclaurin in his *Treatise of Algebra*, may be deduced from these propositions, as every mathematician will easily perceive. This method requires indeed that the least prime divisor should be previously found; and, if the least prime divisor should happen to be a large number, as it is not assignable by any general method, the

investigation of it by repeated tentations may be very tedious. A table therefore of the odd numbers*, in which the Composite numbers should each have its least Prime divisor written over it, would be very useful. But Nichomachus's project of framing a table in which each Composite number should have *all* its divisors written over it, is ridiculous and absurd, on account of the insuperable difficulties which would attend the execution of it.

Feb. 7, 1772.

S. Horsley.

* A table of the odd numbers would be sufficient: for the number 2 is the least prime divisor of every even number; and it is easy, even in the largest numbers, to try whether they are divisible by 2. In our method of notation, this may always be known, by observing the last figure in the expression of the number proposed.

EXCERPTA QUÆDAM

EX

Arithmeticâ Nicomachi

Ad Cribrum Eratosthenis pertinentia.

ἼΙ ὅ τῶν ἡμέσων (α), ὑπὸ Ερατοσθένους, καλεῖται Κόσκιον· ἐπειδὴ ἀναπεφυρμένης τῆς περιστερᾶς λαβόντες καὶ ἀδιακρίτους, ἐξ αὐτῶν [τὰ διαφέροντα ἀλλήλων εἶδη] (β) ταύτην τῇ τῆς ἡμέσεως (γ) μεθόδῳ διαχωρίζομεν, ὥς δι' ὀργάνου ἡ κοσκίνα τινός· καὶ ἰδίᾳ μὲν τὰς πρώτας καὶ ἀσυνθέτας, χωρὶς ὅ τὰς μήκεις εὐρίσκομεν. Ἔστι ὅ ὁ τρόπος τῇ Κοσκίνῃ τοιοῦτος. Ἐκθέμεν τὰς ἀπὸ τριάδος πάντας ἐφεξῆς περιστερὰς, ὥς δυνατὸν μάλιστα ἐπὶ μήκισον εἶχον, ἀρχόμεν ἀπὸ τῆ πρώτης, ἐπισκοπῶν τίνες οἷός τε εἶναι μετρητῶν ἑκάστος· καὶ εὐρίσκω δυνατὸν εἶναι τὸν πρῶτον, ἦτοι τοῦ γ, τὰς δύο μέσας διαλείποντας (δ) μετρητῶν, μέχρις ὅ προχωρεῖν εἰς ἑλωμένη (ε). εἰς ὥς εὖτυχῃ, καὶ εὐκῇ, μετρητῶν, ἀλλὰ τὸν μὲν πρῶτον αὐτῶν κεῖμεν, τῶν ἑστὶ τὸν ἀφ' ἑαυτῆς τὰς δύο μέσας διαλεί-

(α) Mallem ἑρμῆς, etsi, ne quid dissimulem, lectioni receptæ adstipulatur Boethii interpretatio.

(β) Voces uncis inclusas conjecturâ supplevi; quin et sequenti uni ordinem paululum immutavi, pro τῇ ἡμέσῳ μεθόδῳ ταύτῃ, scribendo ταύτη τῇ κ. τ. λ.

(γ) Vocem ἡμέσῳ hic loci retinendam censeo. Locum integrum sic interpretor. “Suam horum indaginem Eratosthenes, Cribrum vocavit. Propterea quod imparibus universis, nullo generum discrimine, in medio collocatis, ipsam procreationem continuam, quo tradidit ille modo, insequendo [id est, procreationis continuæ, Eratosthenis modo, exploratâ lege] species diversas seorsim sistimus, cribro tanquam separatas.”

(δ) Cod. MS. habet διαλείποντα. Wechelius παραλείποντα.

(ε) Ex Cod. MS. pro εἰς ἑλωμένη.

ποῦνα (f), καὶ αὐτὸ τῷ τῷ πρώτῳ ἐν τῷ εἴχῳ καμμένῳ ποσότητι
 μετρήσει· τὰτ' ἐστὶ καὶ αὐτὸ ἐκ τῶν, τρεῖς γὰρ· τὸν δ' αὖτ'
 ἐκείνῳ δύο διαλείποντα, καὶ αὐτὸ τῷ τῷ δευτέρῳ τετραγώνῳ,
 πεντάκις γὰρ· τὸν ὃ περαῖν πάλιν δύο διαλείποντα, καὶ
 αὐτὸ τῷ τῷ τρίτῳ τετραγώνῳ, ἐπτάκις γὰρ· τὸν ὃ ἐπὶ περαῖν
 ὑπὲρ δύο κείδρον, καὶ αὐτὸ τῷ τῷ τέταρτῳ τετραγώνῳ, ἐννέακις
 γὰρ· ἢ ἐπ' ἀπειρεὶν τῷ αὐτῷ τρόπῳ. Ἔτα μετὰ τῶτον, αὖτ'
 ἄλλης ἀρχῆς, ἐπὶ τὸν δεύτερον ἰλθὼν, σκοπῶ τίνας εἰς τὸ
 ἐστὶ μετρεῖν· ἢ εὐρίσκω πάσας τὰς τέσσαρας (g) διαλείποντας·
 ἀλλὰ τὸν μὲν πρώτον, καὶ αὐτὸ ἐν τῷ εἴχῳ πρώτῳ
 τετραγώνῳ ποσότητι· τρεῖς γὰρ· τὸν ὃ δεύτερον, καὶ αὐτὸ τὴν τῷ
 δευτέρῳ· πεντάκις γὰρ· τὸν ὃ τρίτον, καὶ αὐτὸ τὴν τῷ τρίτῳ·
 ἐπτάκις γὰρ· ἢ τῷ τῷ ἐφεξῆς αἰεὶ. Πάλιν ὃ αὖτε, ὁ
 τρίτος, ὁ ζ', τὸ μετρεῖν* παραλαβὼν, μετρήσει τὰς ἑξ' δια-
 λείποντας· ἀλλὰ τὸν μὲν πρώτιστον, καὶ αὐτὸ τὴν τῷ γ' (h)
 ποσότητι, πρώτῳ κείδρον· τὸν ὃ δεύτερον καὶ αὐτὸ τὴν τῷ δ'
 δευτεροταγῆς γὰρ ἑξ' (i). τὸν ὃ τρίτον, καὶ αὐτὸ τὴν τῷ ε',
 τρίτῳ γὰρ ἔχει (k) ἑξ' τάξιν ἐν τῷ εἴχῳ. ἢ, καὶ αὐτὸ τὴν
 αὐτὴν ἀναλογίαν, δι' ὅλον (l) ἀπαρμποδίσας (m) προχαρῆσαι
 σοι τῷτο, ὥστε τὸ μὲν μετρεῖν διαδέχον, καὶ αὐτὸ ἐν τῷ
 εἴχῳ αὐτῶν ἐγκειμένην τάξιν· τὸ ὃ πόσας διαλείποντας,

(f) Locum in Editione Wechelli corruptum, in Cod. MS. mutilum & turbatum, conjecturā, prout potui, sanatum dedi. Editio Wechelli habet τὸν τὰς δύο μίσας ὑπερβαίνοντα. Codex MS. τὸν δύο. τῷ τρεῖς τὸν τρία.

(g) Conjecturā, pro τρεῖς ἀδ.

(h) Litera numeralem γ, conjecturā posui pro voce τρία.

(i) Restitui ex Cod. MS pro ὅλῳ, quæ est Wechelli lectio.

(k) Particulam καὶ omisi.

(l) Wechelium sequor. Cod. MS. habet λαγν, sensu, ut videtur, nullo.

(m) Ex Cod. MS. pro ἀπαρμποδίζον.

* Conjecturā pro μετρεῖν.

καὶ τὸ ἀπὸ δυάδου ἐπ' ἄπειρον εὐτακτον τῶν (n) ἀρτίων
 προκοπὴν, ἢ καὶ τὴν τ' χώρας διπλασίασιν καθ' ἣν ὁ
 μέρων τέτακτο· τὸ δ' ποσάκις, καὶ τὴν τῶν ἀπὸ τετραδέξ.
 περὶ αὐτῶν εὐτακτον ἐπ' ἄπειρον (n) προχώρησιν (p). Ἐάν ἔν
 σημείοις τισὶν ἐπιστήξῃς τῆς ἀριθμῆς, εὐρήσῃς τῆς μέ-
 λαμβάνουσας τὸ μέρειν, ἥτε ἅμα πάντας τ' αὐτὸν πᾶσι
 μέρεσιν, ἔστι δ' ὅτε ἐδὲ δύο τ' αὐτὸν· ἥτε πάντας· οὐ πᾶσι
 τῆς ἐκκεκμηῆς ὑποπίπνουσας μέτρῳ τινὶ αὐτῶν. ἀλλὰ
 τινὰς μὲν παντελῶς διαφεύγουσας τὸ μέρεσθῆναι ὑφ' ἑτισσέν·
 τινὰς δ' ὑφ' ἑνὸς μόνου μέρεσθῆναι· τινὰς δ' ὑπὸ δυο, ἢ καὶ
 πλείονων. Οἱ μὲν ἔν μῆδεσσι (q) μερεσθέντες, ἀλλὰ δια-
 φυγόντες τῷτο, πρῶτοι εἰσὶ καὶ ἀσύνθετοι, ὡς ὑπὸ κοσκίνης
 διακομθέντες. οἱ δ' ὑφ' ἑνὸς μόνου μερεσθέντες, καὶ τὴν
 ἑαυτῶν (r) ποσότητα, ἐν μόνον μόριον ἑτερώνομον ἔξῃσι πρὸς
 τῷ παραινύμῳ. οἱ δὲ ὑφ' ἑνὸς μὲν (s), ἑτέρῳ δὲ ποσότητι, καὶ
 μὴ τῇ ἑαυτῶν, ἢ ὑπὸ δυο ἑμὲ μερεσθέντες, πλείονα ἔξῃσι τῷ
 ἑτερώνομῳ μέρη πρὸς τῷ παραινύμῳ. τῷτο ἔν ἔσονται

(n) Conjectura pro τῇ.

(c) Voces ἐπ' ἄπειρον ex Cod. MS. restitui.

(p) Nempe series numerorum imparium 3, 5, 7, 9, &c. infinite
 protensa, cum numeros impares universos contineat, imparis eu-
 julvis multiplices omnes impares necessario complectitur. Est
 igitur n numerus quilibet impar. In serie 3, 5, 7, &c. infinite
 protensa, habes numeros omnes $n \times 3$, $n \times 5$, $n \times 7$, $n \times 9$, &c.
 Et cum seriei ea Lex sit & Condicio, ut naturali ordine numeri
 impares sequantur, & minor omnis numerus majorem precedat,
 fieri nequit, quin multiplices numeri n cum inter se ordinem
 servant, ut minor quilibet majorem precedat. Primus igitur erit
 $n \times 3$, secundus $n \times 5$, tertius $n \times 7$, & universum, $n \times m$ cum
 habiturus est, inter multiplices, locum, quem numerus m in
 serie.

(q) Ex Cod. MS. vice ἑδμεσ, quæ Wechelii lectio est.

(r) Conjectura pro ἑαυτῶν.

(s) Particulam μὲν ex Cod. MS. restitui.

δεύτεροι ἢ σύνθετοι. Τὸ δὲ εἶναι μέτρον, τὸ καὶνὸν ἀμφοτέρων, ὁ καὶ ἑαυτὸ μὴ δεῦτερον ἢ σύνθετον, πρὶς ἄλλου δὲ πρῶτον ἢ ἀσύνθετον, ἔστιν ὁ ἀπομελόμενος ἀρχαῖος, καλεῖται τὴν ἑαυτοῦ ποσότητι πρώτην ἢ ἀσυνθέτην μέτρησιν. ταύτης εἴτις [τάτη τῷ τρόπῳ] (1) φωνήμεθα, συγκροτοῦμεν πρὸς ἄλλον ὁσπύτως τὴν φωνὴν ἐστίν, αἰσθητὴ ἢ οὐ, φωνή γὰρ ἐκ τῆς γ (u) κατὰ τὴν ἑαυτοῦ ποσότητα μέτρησιν. τρεῖς γὰρ εἰ συγκροτοῦμεν πρὸς τὴν φωνήν γὰρ ἔστι (x) ἐκ τῆς ι, καὶ αὐτὴ τὴν ἑαυτοῦ ποσότητα μέτρησιν. ταυτάρως γὰρ κοινὸν μέτρον ταύτοις ἢκ' ἐστίν, εἰ μὴ μόνη ἡ Μονάς.

(1) Voces τάτη τῷ τρόπῳ conjecturā supplevi.

(u) Literam numeralem γ pro voce prima quae apud Wechelium legitur, ex Cod. MS. restitui.

(x) Voces γὰρ καὶ ἐστίν ex Cod. MS. restitui.

Ex Arithmetica Boethii.

Lib. I. c. xvii.

GENERATIO autem ipsorum atque ortus hujusmodi investigatione colligitur, quam scilicet Eratosthenes Cribrum nominabat; quod cunctis imparibus in medio collocatis, per eam, quam tradituri sumus, artem, qui primi, quive secundi, quique tertii generis videantur esse distinguuntur. Disponantur enim a ternario numero cuncti in ordinem impares, in quamlibet longissimam porrectionem 3. 5. 7. 9. 11. 13. 15. 17. 19. 21. 23. 25. 27. 29. 31. 33. 35. 37. 39. 41. 43. 45. 47. 49. His igitur ita dispositis, considerandum, primus numerus quem eorum, qui sunt in ordine positi, primum metiri possit: sed, duobus præteritis, illum, qui post eos est positus, mox metitur: et, si post eundem ipsum quem mensus est, alii duo transmissi sunt, illum, qui post duos est, rursus metitur: et, eodem modo si duos quis reliquerit, post eos qui est, a primo numero metiendus est; eodemque modo, relictis semper duobus, a primo, in infinitum pergentes metientur. Sed id non vulgo neque consuevit. Nam primus numerus illum, qui est post duos secundum se locatos, per suam quantitatem metitur: ternarius enim numerus ter * 9 metitur. Si autem post novenarium duos reliquero, qui mihi post illos incurre-

* Conjectura pro *tertio*.

rit, a primo metiendus est, per secundi imparis quantitatem; id est, per quinarium: nam si post 9 duos relinquam, id est 11 & 13, ternarius numerus 15 metietur, per secundi numeri quantitatem, id est, per quinarium; quoniam numerus ternarius 15 quinquies metitur. Rursus, si a quindenario interchoans duos intermisero, qui posterior positus est, ejus primus numerus mensura est, per tertii imparis pluralitatem: nam si post 15 intermisero 17 & 19, incurrit 21, quem ternarius numerus secundum septenarium metitur; 21 enim numeri ternarius septima pars est: atque hoc in infinitum faciens, reperio primum numerum, si binos intermisero, omnes sequentes post se metiri, secundum quantitatem positorum ordine imparium numerorum. Si vero quinarium numerus, qui in secundo loco est constitutus, velit^b quis, cujus prima ac deinceps sit mensura, invenire, transmissis quatuor imparibus, quintus ei quem metiri possit, occurrit. Intermittantur enim quatuor impares, id est, 7 & 9, & 11 & 13, post hos est quintus decimus quem quinarium metitur, secundum primi scilicet quantitatem, id est, ternarii; quinque enim 15 ternarii metiuntur: ac deinceps, si quatuor intermittat, eum qui post illos locatus est, secundus, id est, quinarium, sui quantitate metitur: nam post quindecim intermissis 17 & 19, & 21 & 23, post eos 25 reperio, quos quinarium scilicet numerus sua pluralitate metitur; quinquies enim quinario multiplicato, 25 succrescunt; si vero post hunc quilibet quatuor intermittat, eandem ordinis servatâ

^b Conjecturâ pro *vel*.

^a Conjecturâ pro *tertio*.

constantia, qui eos sequitur, secundum tertii, id est, septenarii numeri summam, a quinario metitur: atque hæc est infinita processio. Si vero tertius numerus quem metiri possit exquiritur, sex in medio relinquentur; & quem septimum ordo monstraverit, hic per primi numeri, id est, ternarii quantitatem metiendus est: et post illum, sex aliis interpositis, quem post eos numeri series dabit, per quinarium, id est, per secundum, tertii cum mensura percurreret: si vero alios rursus sex in medio quis relinquat, ille, qui sequitur, per septenarium ab eodem septenario metiendus est; id est, per tertii quantitatem; atque hic usque in extremum ratus ordo progreditur. Suscipient ergo metiendi vicissitudinem, quemadmodum sunt in ordine naturaliter impares constituti: metientur autem, si per pares numeros, a binario inchoantes, positos inter se impares, ratâ intermissione, transiliant; ut primus duos, secundus quatuor, tertius sex, quartus octo, quintus decem⁴: vel si locos suos conduplicent, & secundum duplicationem terminos intermittant; ut ternarius, qui primus est numerus, & Unus, omnis enim primus Unus est, bis locum suum multiplicet, faciatque bis unum; qui cum duo sint, primus duos medios transeat. Rursus secundus, id est, quinarium, si locum suum multiplicet, 4 explicabitur: hic quoque quatuor⁵ intermittat. Item si septenarius, qui tertius est, locum suum duplicet, sex creabit; bis enim 3 senarium jungunt: hic ergo in ordine⁶ sex relinquat. Quartus quoque, si locum

⁴ Conjecturâ restitui pro 12.

⁵ Conjecturâ pro 4.

⁶ Conjecturâ pro *ordinem*.

sum duplicet, 8 succrescent; ille quoque octo transiliat: atque hoc quidem in cæteris perspicendum. Modum autem mentionis, secundum ordinem collocatorum, ipsa series dabit. Nam primus primum quem numerat, secundum primum numerat^a, id est, secundum se; & secundum primus quem numerat, per secundum numerat^v, & tertium per tertium, & quartum item per quartum. Cum autem secundus mentionem^b susceperit, primum quem numerat secundum primum metitur; secundum vero quem numerat per se, id est, per secundum; & tertium per tertium: & in cæteris eadem similitudine mensura constabit. Illos^c ergo si respicias, vel qui alios mensi sunt, vel qui ipsi ab aliis metiuntur, invenies omnium simul communem mensuram esse non posse, neque ut omnes quemquam alium simul numerent; quosdam autem ex his ab alio posse metiri, ita ut ab uno tantum numerentur^k; alios vero, ut etiam a pluribus; quosdam autem, ut præter Unitatem eorum nulla mensura sit. Qui ergo nullam mensuram præter Unitatem recipiunt, hos Primos & Incom-

^a Conjectura pro 8.

^b Pro *numerat* mallet in utroque loco, *metitur*, ut aliud sit *numerare*, aliud *metiri*, & sensus sit, "That which the first number [of the Series] counts the first [of its multiples], it measures by the first [of the Series], i. e. by itself. That which it counts the second [of its multiples], it measures by the second [number in the Series]." Sic enim infra legimus de Numero ordine secundo, "primum quem numerat secundum primum metitur."

^c Conjectura, pro *mentionem*.

^k Conjectura, pro *alios*.

^{*} Ang. "But so as to be counted in among the multiples of one number only."

positos judicamus; qui vero aliquam mensuram præter Unitatem, vel alienigenæ partis vocabulum fortiuntur, eos pronunciemus Secundos atque Compositos. Tertium vero illud genus, per se Secundi & Compositi, Primi vero & Incompositi ad alterutrum comparati, hâc inquisitor ratione reperiet. Si enim quoslibet primos¹ numeros, secundum suam in semetipsos multiplices quantitatem, qui procreantur, ad alterutrum comparati, nullâ mensurâ communione junguntur: 3^m enim & 5, si multiplices, 3 ter^a 9 faciunt, & quinquies 5 reddunt 25. His igitur nulla est cognatio communis mensuræ. Rursus 5 & 7 quos procreant, si compares, hi quoque incommensurabiles erunt: quinquies enim 5, ut dictum est, 25, septies 7 faciunt 49; quorum mensura nulla communis est, nisi forte omnium horum procreatrix & mater Unitas^o.

¹ Conjectura pro *illos*.

^a Conjecturâ, pro *tres*. * Conjecturâ pro *tres tertis*.

* Sed cave credas, Lector, numeros inter se primos nullos dari præter Primorum Quadratos.

XXIII. *A Letter from Mr. Christopher Gullet to Matthew Maty, M. D. Sec. R. S. on the Effects of Elder, in preserving Growing Plants from Insects and Flics.*

Tavistock (Devon) August 11, 1771.

S I R,

Read May 14, 1771. **I** SHOULD not presume to trouble you as a member of the Royal Society with the following letter, did not the subject seem to promise to be of great public utility. It relates to the effects of Elder;

Sambucus fructu in um'ella nigro.

1st. In preserving cabbage plants from being eaten or damaged by caterpillars.

2d. In preventing blights, and their effects on fruit and other trees.

3d. In the preservation of crops of wheat from the yellows, and other destructive insects.

4th. Also in saving crops of turnips from the fly, &c. &c.

1st, I was led to my first experiments, by considering how disagreeable and offensive to our olfactory nerves the effluvia emitted by a brush of green-elder

elder leaves are, and from thence, reasoning how much more so they must be to those of a butterfly, whom I considered as being as much superior to us in delicacy as inferior in size. Accordingly I took some twigs of young elder, and with them whipt the cabbage plants well, but so gently as not to hurt them, just as the butterflies first appeared; from which time, for these two summers, though the butterflies would hover and flutter round them like gnomes or sylphs, yet I could never see one pitch, nor was there I believe a single caterpillar blown, after the plants were so whipt; though an adjoining bed was infested as usual.

2d. Reflecting on the effects abovementioned, and considering blights as chiefly and generally occasioned by small flies, and minute insects, whose organs are proportionably finer than the former, I whipt the limbs of a wall plumb tree, as high as I could reach; the leaves of which were preserved green, flourishing, and unhurt, while those not six inches higher, and from thence upwards, were blighted, shrivelled up, and full of worms. Some of these last I afterwards restored by whipping with, and tying up, elder among them. It must be noted, that, this tree was in full blossom at the time of whipping, which was much too late, as it should have been done once or twice before the blossom appeared. But I conclude from the whole, that if an infusion of elder was made in a tub of water, so that the water might be strongly impregnated therewith, and then sprinkled over the tree, by a hand engine, once every week or fortnight, it would effectually
answer.

answer every purpose that could be wished, without any possible risk of hurting the blossoms or fruit.

3d. What the farmers call the yellows in wheat, and which they consider as a kind of mildew, is in fact, as I have no doubt but you well know, occasioned by a small yellow fly with blue wings, about the size of a gnat. This blows in the ear of the corn, and produces a worm, almost invilible to the naked eye; but being seen through a pocket microscope, it appears a large yellow maggot of the colour and gloss of amber, and is so prolific that I last week distinctly counted 41 living yellow maggots or insects, in the husk of one single grain of wheat, a number sufficient to eat up and destroy the corn in a whole ear. I intended to have tryed the following experiment sooner; but the dry hot weather bringing on the corn faster than was expected, it was got and getting into fine blossoms ere I had an opportunity of ordering as I did; but however the next morning at daybreak, two servants took two bushes of elder, and went one on each side of the ridge from end to end, and so back again, drawing the elder over the ears of corn of such fields as were not too far advanced in blossoming. I conceived, that the disagreeable effluvia of the elder would effectually prevent those flies from pitching their tents in so noxious a situation; nor was I disappointed, for I am firmly persuaded that no flies pitched or blowed on the corn after it had been so struck. But I had the mortification of observing the flies (the evening before it was struck) already on the corn (six, seven or eight, on a single ear) so that what damage hath accrued, was done before

before the operation took place; for, on examining it last week, I found the corn which had been struck pretty free of the yellows, very much more so than what was not struck. I have, therefore, no doubt but that, had the operation been performed sooner, the corn would have remained totally clear and untouched. If so, simple as the process is, I flatter myself, it bids fair to preserve fine crops of corn from destruction, as the small insects are the crops greatest enemy. One of those yellow flies laid at least eight or ten eggs of an oblong shape on my thumb, only while carrying by the wing across three or four ridges, as appeared on viewing it with a pocket microscope.

4th. Crops of turnips are frequently destroyed, when young, by being bitten by some insects, either flies or fleas; this I flatter myself may be effectually prevented, by having an elder bush spread so as to cover about the breadth of a ridge, and drawn once forward and backward by a man over the young turnips. I am confirmed in this idea, by having struck an elder bush over a bed of young collyflower plants, which had begun to be bitten, and would otherwise have been destroyed by those insects; but after that operation it remained untouched.

In support of my opinion, I beg leave to mention the following fact from very credible information, that about eight or nine years ago this county was so infested with **cock chaffers** or oakwebs, that in many parishes they eat every green thing, but elder; nor **left a green leaf** untouched besides elder bushes, which alone remained green and unhurt, amid the general devastation of so voracious a multitude. On
reflecting

reflecting on these several circumstances, a thought suggested itself to me, whether an elder, now esteemed noxious and offensive, may not be one day seen planted with, and entwining its branches among, fruit trees, in order to preserve the fruit from destruction of insects: and whether the same means which produced these several effects, may not be extended to a great variety of other cases, in the preservation of the vegetable kingdom.

The dwarf elder (*ebulus*) I apprehend emits more offensive effluvia than common elder, therefore must be preferable to it in the several experiments.

On mentioning lately to Sir Richard W. Bampfylde, one of the representatives of this county, my observations on the corn crops, and the effects of the elder, &c. he persuaded me to publish them, which in some measure determined my taking this step, of transmitting them to a Society incorporated for promoting the knowledge of natural things, and useful experiments, in which they have so happily and amply succeeded, to the unspeakable advantage and improvement both of the old and new world. I have the honour to subscribe myself,

SIR,

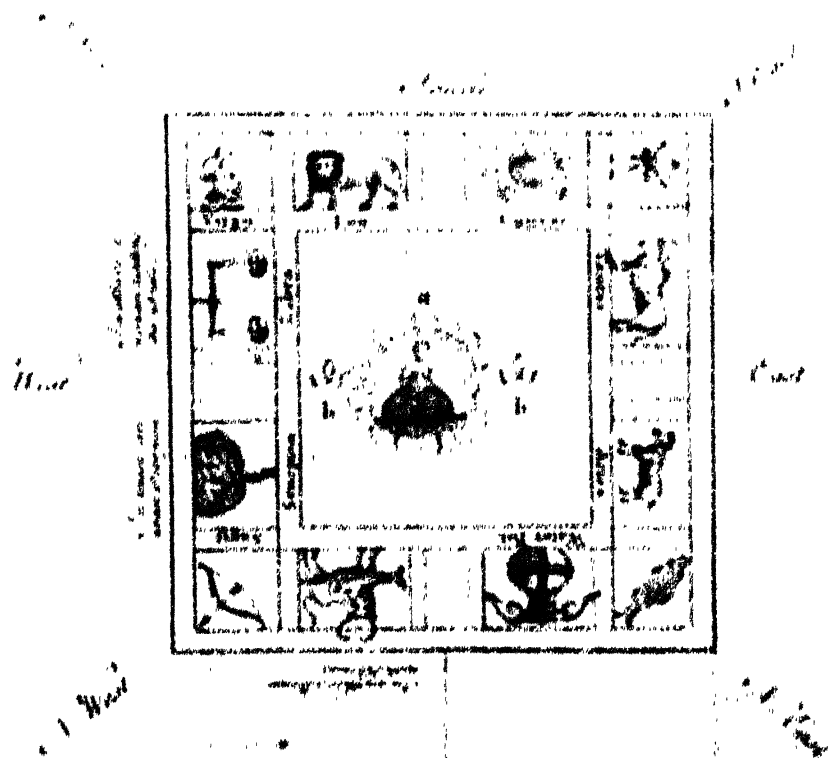
Your most obedient,

humble Servant,

Chr. Gullett.

THE UNIVERSAL SYMBOLS OF THE
 VERDADIAN OF THE MADURAH COUNTRY

1840



a. Symbol of the Universal Duty.
 bb. Two books of Ten to suspend a hand of change on which the
 Duty or army often out when exhibited to the above.

XXIV. *A Letter from John Call, Esq; to Nevil Maskelyne, F. R. S. Astronomer Royal, containing a Sketch of the Signs of the Zodiac, found in a Pagoda, near Cape Comorin in India.*

S I R,

Read May 14,
1772. **A**S a member of the Royal Society, and one whose study is particularly directed to the motions of the heavenly bodies, I think you the most proper person to whom I can send the inclosed sketch [Tab. X.], which I drew with a pencil, as I lay on my back resting myself during the heat of the day, in a journey from Madurah to Twinwelly, near Cape Comorin. And I send it to you rather in the original, as I then sketched it off, than in any more complete form, lest it should thereby have more the appearance of composition, and leave not so strong an impression of antiquity, as it made on me when I discovered it.

After such a discovery, I searched in my travels many other pagodas, or choultrys, for similar carvings; but, to the best of my remembrance, never found

but one more equally complete, which was on the ceiling of a temple, in the middle of a tank before the pagoda of Teppecolum, near Mindurah, of which tank and temple Mr. Ward, painter in Broad-street, near Carnaby-market, hath a drawing; but I have often met with the several parts in detached pieces.

From the correspondence of the signs of the zodiac which we at present use, and which we had, I believe, from the Arabians or Egyptians, I am apt to think that they originally came from India, and were in use among the Bramins, when Zoroaster and Pythagoras travelled thither, and consequently adopted and used by those travellers: and as these philosophers are still spoken of in India, under the names of Zerdhurst and Pythagore, I should also hazard another idea, that the worship of the cow, which still prevails in India, was transplanted from thence to Egypt. But this is only conjecture; and it may with almost equal probability be said, that Zoroaster or Pythagoras carried that worship to India.

However, I think there is an argument still in favour of India for its antiquity, in point of civilization and cultivation of the arts and sciences; for it is hardly in dispute that all these improvements came from the east to the west; and, if we may be allowed to draw any conclusions from the immense buildings now existing, and from the little of the inscriptions, which can be interpreted on several of the choultrys and pagodas, I think it may safely be pronounced, that no part of the world has more marks of antiquity for arts, sciences, and civilization,

tion, than the peninsula of India, from the Ganges to Cape Comorin; nor is there in the world a finer climate, or face of the country, nor a spot better inhabited, or filled with towns, temples, and villages, than this space is throughout, if China and parts of Europe are excepted.

I think the carvings on some of the pagodas and choultrys, as well as the grandeur of the work, exceeds any thing executed now-a-days, not only for the delicacy of the chissel, but the expence of construction, considering, in many instances, to what distances the component parts were carried, and to what heights raised. If Mr. Kittle the painter, now in India, should have time and opportunity, after he hath made his fortune by portrait drawing, it would be a great addition to his reputation, and well worth his pains, to investigate the nature of the Indian architecture and carving, by painting some of the most curious buildings, or parts of pagodas. The great obstacle to ascertaining dates, or historical events, is the loss of the Sans-Skirrit language, and the confinement of it to the priesthood. I should have taken some pains to have collected many things; but the number of revolutions and occupations which happened always prevented me.

I also commit to your inspection the * manuscripts of Mr. Robins, which he gave me at his death;

* These I communicated to the Royal Society, together with this letter; but being examined by myself, Mr. Raper, Mr. Cavendish, and Mr. Worsley, at the desire of the Society, they were not found to contain any thing material more than has been already printed; excepting a treatise on military discipline; which, if it should be thought of use, may be inserted in the next edition of his works. N. M.

I believe most of them have been printed, but if there are any which have not, or that can amuse you or instruct others, you are welcome to use them as you please: I only wish they may contain any thing useful. While he lived, I pursued those studies; but, soon after his death, new scenes arose, and engaged me more in practical service, than allowed me time for theory, or experiments. I am, however, a constant well-wisher to the progress of arts and sciences, as well as study; and very much,

S I R,

Your obedient,

humble servant,

Jn^o Call.

XXV. *An Account of the Flowing of the Tides in the South Sea, as observed on board His Majesty's Bark the Endeavour, by Lieut. J. Cook, Commander, in a Letter to Nevil Maskelyne, Astronomer Royal, and F. R. S.*

Mile-end, February 5, 1772.

Reverend Sir,

Read May 21, 1772. **I** Here send you the few observations I made on the tides in the South Sea, to which I have only to add, that, from many circumstances and observations, I am fully convinced that the flood comes from the southward, or rather from the S. E. I am,

S I R,

Your most obedient,

humble servant,

J. Cook.

Names

| Names of places where observed. | Lat.
South. | Long.
West. | New and full
Moon. | |
|--|----------------|----------------|-----------------------|-----------------|
| | | | High
water. | Rise &
fall. |
| | | | H. M. | F. In. |
| Success Bay in Strait le Maire | 54 45 | 66 4 | 4 30 | 5 0 |
| Lagoon Island | 18 47 | 139 28 | 0 30 | ... |
| Matuvai Bay, Otaheite | 17 29 | 149 30 | 0 30 | 0 11 |
| Tolaga Bay, East coast of New Zealand | 38 22 | 181 14 | 0 0 | 5 0 |
| Mercury Bay, N. E. ditto | 36 48 | 184 4 | 7 30 | 7 0 |
| River Thames, ditto | 37 12 | 184 12 | 9 0 | 10 0 |
| Bay of Islands, ditto | 35 14 | 185 36 | 8 0 | 7 0 |
| Queen Charlotte's Sound, Cook's Strait
New Zealand | 41 0 | 184 45 | 9 30 | 7 6 |
| Admiralty Bay, in ditto | 41 45 | 185 11 | 10 0 | 7 0 |
| Botany Bay, coast of New South-Wales | 34 0 | 158 37 | 8 0 | 4 6 |
| Bustard Bay, ditto | 34 30 | 158 30 | 8 0 | 8 0 |
| Thirly Sound, ditto | 35 5 | 160 24 | 11 0 | 16 0 |
| Endeavour River, ditto | 15 26 | 114 48 | 9 30 | 9 0 |
| Endeavour's Strait, which divides New
Guinea from New Holland | 10 37 | 118 45 | 1 30 | 11 0 |

XXVI. *An Account of a new Electrometer, contrived by Mr. William Henly, and of several Electrical Experiments made by him, in a Letter from Dr. Priestley, F.R.S. to Dr. Franklin, F. R. S.*

DEAR SIR,

Read May 28, 1772. **I** THINK myself happy in an opportunity of giving you a species of pleasure, which I know is peculiarly grateful to you as the father of modern electricity, by transmitting to you an account of some very curious and valuable improvements in your favourite science. The author of them is Mr. Henly, in the Borough, who has favoured me with the communication of them, and has given me leave to request, that you would present them to the Royal Society.

In my history of electricity, and elsewhere, I have mentioned a good electrometer, as one of the greatest desiderata among practical electricians, to measure both the precise degree of the electrification of any body, and also the exact quantity of a charge before the explosion, with respect to the size of the electrified body, or the jar or battery with which it is connected, as well as to ascertain the moment of time, in which the electricity of a jar changes, when, without making an explosion, it is discharged by giving.

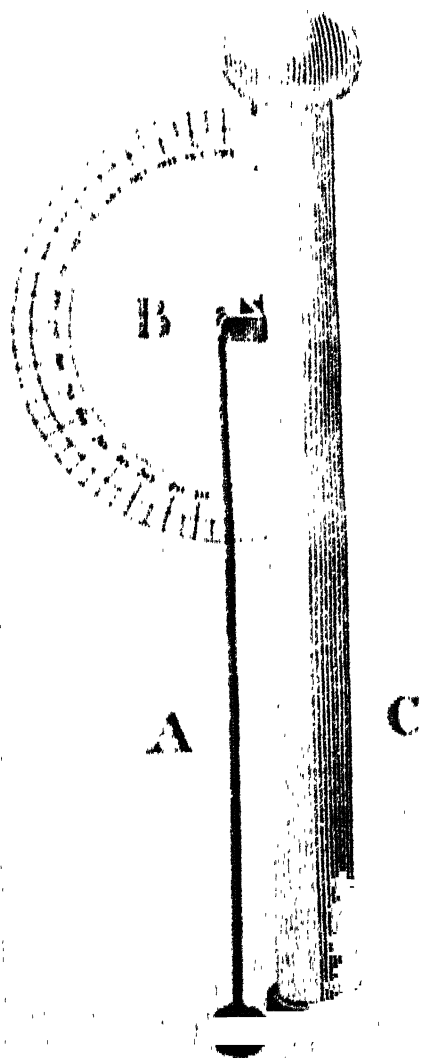
giving it a quantity of the contrary electricity. All these purposes are answered, in the most complete manner, by an electrometer of this gentleman's contrivance, a drawing of which I lend you along, with the following description.

The whole instrument is made of ivory or wood, [Tab. XI.] (*a*) is an exceeding light rod, with a cork ball at the extremity, made to turn upon the center of a semicircle (*b*), and so as always to keep pretty near the limb of it, which is graduated: (*c*) is the stem that supports it, and may either be fixed to the prime conductor, or be let into the brass knob of a jar or battery, or set in a stand, to support itself.

The moment that this little apparatus is electrified, the rod (*a*) is repelled by the stem (*c*), and consequently begins to move along the graduated edge of the semicircle (*b*); so as to mark with the utmost exactness, the degree in which the prime conductor, &c. is electrified, or the height to which the charge of any jar or battery is advanced; and as the materials of which this little instrument is made are very imperfect conductors, it will continue in contact with any electrified body, or charged jar, without dissipating any of the electricity.

If it should be found, by trial in the dark, that any part of this instrument contributes to the dissipation of the electric matter, (which, when the electrification was very strong, I once observed mine to do) it should be baked * a little, which will presently prevent it. If it is heated too much, it will not receive electricity readily enough, and then the motion of the index will not correspond with sufficient

* Warmed a little, to dry off the damp, particularly from the index.



• The distance of point B by experience to be the most perfect, when
 the instrument is made as of. It is made very smooth with a very
 paper. • The ball should be with the greatest plate & heavy as
 the distance of that substance as more light than we need.

exactness, to the degree in which the body to which it is connected is electrified; but this inconvenience is easily remedied, by moistening the stem and the index, for the semicircle cannot be too dry.

I find by experience, that this electrometer answers all the purposes I have mentioned, with the greatest ease and exactness. I am now sure of the force of any explosion before a discharge of a jar or battery, which I had no better method of guessing at before, than by presenting to them a pair of Mr. Canton's balls, and observing their divergency at a given distance; but the degree of divergency was still to be guessed at by the eye, and the balls can only be applied occasionally; whereas this instrument, being constantly fixed to the prime conductor or the battery, shews, without any trouble, the whole progress of the charge; and, remaining in the same situation, the force of different explosions may be ascertained with the utmost exactness before the discharge.

If a jar be loaded with positive electricity, and I want to know the exact time when, by attempting to charge it negatively, it first becomes discharged, I see every step of its approach to this state by the falling of the index; and the moment I want to seize, is the time when it has got into a perpendicular situation, which may be observed, without the least danger of a mistake. Accordingly I find that, in this case, not the least spark is left in the jar. If I continue the operation, the index, after having gained its perpendicular position, begins to advance again, and thereby shews me the exact quantity of the opposite electricity that it has acquired.

Considering the admirable simplicity, as well as the great usefulness of this instrument, it is something surprizing that the construction should not have occurred to some electrician before this time. Nollet's and Mr. Waits's invention of threads, projecting shadows upon a graduated board, resembled this apparatus of Mr. Henly's, but was a poor and awkward contrivance in comparison with it; nor was Richman's gnomon, though a nearer approach to this construction, at all comparable to it; and the ingenious author of it had no knowledge of either of those methods when he hit upon this.

I have made a receptacle for this instrument in my prime conductor, and I have also a pedestal in which I can fix it; and by means of which I can very conveniently place it on the wires of a battery.

In either of those situations it answers almost every purpose of an electrometer, without removing it from its place.

I doubt not that you and all other electricians will join with me in returning our hearty thanks to Mr. Henly for this excellent and useful instrument.

Many of the effects of my battery, in breaking of glass, and tearing the surface of bodies, Mr. Henly performs by a single jar, only increasing the weight with which the bodies are pressed, while the explosion is made to pass close under them.

By this means he raises exceeding great * weights, and shatters strong pieces of glass into thousands of the smallest fragments; he even reduces thick plate glass by this means to an impalpable powder. But

* Frequently six pounds Troy.

what is most remarkable is, that when the pieces of glass are thick, and strong enough to resist the shock, they are marked by the explosion, with the most lively and beautiful colours, generally covering the space of about an inch in length, and half an inch in breadth.

In some of the pieces which he was so obliging as to send me, these colours lie all intermixed and confused; but in others I observe them to be disposed in prismatic order, in lines parallel to the course of the explosion, and in some (as N° 1.) I have counted three or four distinct returns of the same colour.

He has lately informed me, that, since he sent me this piece, he has struck these prismatic colours into another mass of glass, in a still more vivid and beautiful manner, the colours shooting into one another. This effect, he says, was produced by making a second explosion, without moving any of the apparatus after the first.

When the glass in which these colours are fixed is examined, it is evident that the surface is shattered into thin plates, and that these give the colours, the thickness of them varying regularly, as they recede from the path of the explosion.

In the middle of these coloured spots (as in N° 2.) some of these thin plates, or scales, are struck off, I suppose by the force of the explosion; and with the edge of a knife they are all easily scraped away, when the surface of the glass is left without its polish (as in N° 3.)

The piece of glass on which I have marked these numbers, as well as that on which he has struck the

colours in a still more beautiful manner, Mr. Henly will present to the Royal Society, for the inspection of the members.

Besides these improvements, Mr. Henly has likewise, in a very ingenious manner, diversified several of the more entertaining experiments in electricity, particularly in his imitation of the effects of earthquakes by the lateral force of explosions; and he has also hit upon several curious facts, that, unknown to him, had been observed before by others: the following particular, however, I believe is new, exciting a stick of sealing wax, and using a piece of tin foil for the rubber, he found that it would electrify positively, as well as glass rubbed with silk and amalgama.

Wishing we had more such fellow labourers as Mr. Henly, I am,

DEAR SIR,

Your obliged

humble servant,

Leeds,
Oct. 26, 1770.

J. Priestley.

Read May 23, 1772.

XXXVII. *Meteorological Observations at Ludgvan in Mount's-Bay, Cornwall, 1771: By William Borlase*, D.D. F.R.S. Communicated by Dr. Jeremiah Milles, Dean of Exeter, and F.R.S.*

| Month. | Barometer. | State of the Weather and Wind. | Fahrenheit's Thermom. | Omb. |
|---------|-----------------------------------|--|---|----------------------------|
| January | Highest 23 30.5
Lowest 19 29.0 | The 1st at night a violent storm, and rain till midnight. On the 2d at 8 P. M. a violent storm, which continues all night; stormy the 3d and 4th; ditto the 26th at night; wind Westerly. On the 10th at night, after hail showers, a great fall of snow; the 11th great snow falling, with stormy blasts; the 12th deep snow and more falling, with frost; deep snow, and hard frost, the 13th, 14th, 15th, 16th, 17th, 18th, and 19th, snow lying deep, but the frost more gentle and the thaw came on; the 20th P. M. it thawed fast; on the 21st in the afternoon, the frost and snow was all gone; the rest of the month mostly mists with some hard showers of rain. Wind, during the storms, mists, and rain, Westerly for 16 days; during the cold, East, and East North East. | Monthly Med. of heat for each day.
Highest 1 50 } 39.5
Lowest 17 27½ } 39.1 | Inches.
3.000
3.7000 |

* This is the last entry of this kind, which the Society will receive from the excellent author of the Natural History of Cornwall, and several other learned works; death having, though at an advanced age, put a period to a life divided between the pursuit of useful and experimental knowledge, and the faithful discharge of every moral and religious duty. M. M.

Month

| Month. | Barometer. | State of the Weather and Wind. | Fahrenheit's Thermom. | Omb. |
|--------|-------------------------------------|--|--|------|
| June | Highest 3 30.15
Lowest 1 29.54 | Calm, 3 ^d , 4 th , 5 th , 6 th , 7 th , 8 th , 9 th , 10 th , 11 th , 12 th , 13 th , 14 th , 15 th , 20 th , 21 st , 22 ^d , 23 ^d , 24 th , 25 th , 26 th , 27 th , 28 th , 29 th , 30 th (in all 24 days) the wind variable and mixed, but the weather remarkably settled, fair, and pleasant. On the 28 th however, there was most violent thunder, lightning, and a flood of rain, at the towns of Penryn and Falmouth, 20 miles distant from Mount's-Bay to the East; but in Mount's-Bay, the air was cloudy, and only some distant thunders; the lightning was scarce visible, and not a drop of rain. | Med. Inches.
Highest 27 71½ }
Lowest 3 49 } 58 2 ½ } 0,200 | |
| July | Highest 14 30.15
Lowest 31 29.55 | Calm, 1 st , 2 ^d , 3 ^d , 4 th , 5 th , 6 th , 7 th , 10 th , 13 th , 14 th , 15 th , 16 th , 17 th , 18 th , 23 ^d , 24 th , 25 th , 27 th , 28 th , 29 th , 30 th , the rest mixed. Wind 24 days from the West, mixed mostly with the South. N. B. As we had a most unusual run of dry weather here in Cornwall, in other parts of the world, they had altogether as extraordinary a glut of rain. See, for particulars, the public papers, from Berlin, Dresden, Hamburg, and Vienna, &c. in Europe, and from Virginia in North America, where their inundations have not been remembered so destructive. | Highest 17 72 }
Lowest 31 54½ } 61 5 ½ } 0,720 | |

Month.

| Month. | Barometer. | State of the Weather and Wind. | Fahrenheit's Thermom. | Ombr. |
|-----------|-------------------------------------|--|---|--------|
| August | Highest 29 30.0
Lowest 19 29.40 | Calm, 1st, 2d, 3d, 4th, 5th, 6th, 15th, 17th, 22d, 23d, 26th, 27th, 28th, 29th, 30th, 31st; the rest mild, foggy, and rains, with some gales interspersed. Wind 25 days from the West, mixed, nearly equal with North and South. | Highest 31 65
Lowest 1 54 } Med. }
58.4
58.7 | 3.633 |
| September | Highest 28 30.6
Lowest 20 29.30 | Calm, 2d, 3d, 5th, 6th, 10th, 11th, 12th, 13th, 14th, 15th, 16th, 17th, 18th, 24th, 25th, 26th, 28th; the 19th, 20th, 21st, 22nd, 23rd, 24th, 25th, 26th, 27th, 28th, 29th, 30th, 31st, cloudy, windy, mixed with rain. Wind mostly from the West, mixed with the North. | Highest 1 61
Lowest 20 44 }
55.5
55.5 | 3.400 |
| October | Highest 31 30.35
Lowest 13 28.85 | Calm, only 8 days A. M. only 2 days P. M.; the rest rainy, windy, stormy. A violent storm on the 13th and 14th; the extrem on the 12th at 10 P. M. wind South West. N. B. on the 13th, at Caton near Lancaster, happened the greatest inundation in the memory of man. Wind 23 days from the West, mixed mostly with the South. | Highest 8 57
Lowest 31 43 }
51.7
45.5 | 4.550 |
| November | Highest 18 30.40
Lowest 11 29.40 | Calm, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 9th, 13th, 18th, 19th, 20th, 22d, 25th, 26th, 27th. On the 11th, 12th, 15th, 16th, stormy with rain and showers. Wind 24 days from the West, mixed mostly with the South. N. B. This month was very dry in Cornwall; but by the 17th, such incessant rains in the middle of it, from the 15th to the 17th, such inundations happened at Newcastle, Dartmouth, Barnard-castle, and near Carlisle, by the breaking of Salway-moat, as have never been known to de- | Highest 16 55
Lowest 10 39 }
47.7
44.5 | 1.450 |
| | | | | Month. |

| Month. | Barometer. | State of the Weather and Wind. | Fahrenheit's Thermom. | Omb. |
|----------|-----------------------------------|---|---|------|
| December | Highest 30 30.4
Lowest 7 28.40 | Calm, 1st, 2d, 8th, 13th, 17th, 18th. On Friday the 16th (new-moon at 8 A. M. wind South by East, and South East) about 8 P. M. it blew a violent storm, and setting full into Mount's-Bay at spring-tide, the sea was 60 high, and furious, that it demolished houses, cellars, boats, and walls, wherever it reached. The four towns on the shore all suffered; and it has been calculated, that not less than 5000 <i>£</i> . damage was done here that night. Besides ships lost. It reached to the Eastward; and at Plymouth, about 60 miles off, they reckoned the tide was higher by ten feet than usual. The remainder of this month was showery, rainy, windy. Wind Westerly 24 days, mixed mostly with the South. | Highest 11 54
Lowest 30 36 } Med. inches.
46.294
5.350 | |

The whole Rain fallen in this Year 1771, } 30.153
at Ludgvan, a very dry Year, } 10.000

XXVIII. *Account of several Quadrupeds from Hudson's Bay**, by Mr. John Reinhold Forster, P. R. S.

Read May 21, 1777.

1. Arctic Fox, Penn. Synops. of Quadr. p. 155. n. 113. *Canis Lagopus*, Linn.

Severn River.

A most beautiful specimen in its snowy winter furr; this animal seems to be lower on its legs than the common fox, and is prodigiously well secured against the intense cold of the climate, by the thickness and length of its hairs, which are at the same time as soft as silk.

* Among the occasional advantages, which the observations of the 18th Transit of Venus have procured, that of receiving useful information from, and sending correspondencies to, several parts of the world, is not the least considerable. From the factory at Hudson's Bay, the Royal Society were favoured with a large collection of uncommon quadrupeds, birds, fishes, &c. together with some account of their names, place of abode, manner of life, use, &c. by Mr. Graham, a gentleman belonging to the settlement on Severn River; and the governors of the Hudson's Bay Company have most obligingly sent orders, that these communications should be from time to time continued. The descriptions contained in the following papers were prepared and given by Mr. Forster, before his departure on an expedition, which will probably open an ample field to the most important discoveries. M. M.

The

The account sent along with it from Severn River says, that these white foxes are silly, inoffensive animals; and are known to stand by, whilst a trap is baited for them, into which they put their heads immediately: they will, when pinched by hunger, devour those of their own kind, which are already caught in these traps. But the most curious circumstance is, their migration to the Northward and the Eastern coasts of the bay; for though a few of them are caught every year near York fort and Churchill river, yet, once in three or four years, they come in great numbers; and several hundred of their furs are sent to England in that plentiful seasons, which always begins in November, and ends in April. The specimen sent is full grown, and its furr quite in season.

2. LESSER OTTER. Penn. Syn. Quadr. p. 239. n. 174. *Mustela Lutreola* Linn. Syst. Nat. 66. Faun. Succ. N° 13.

Severn River.

I am still dubious, whether this animal ought to be looked upon as the same with the lesser otter of Europe and Asia; many circumstances seem to prove this identity; but some, such as the want of webs, which I could not discover between the toes, and the white spot on the neck, will not admit of it. I have, therefore, subjoined a description of this creature at the end of this article. The natives of Hudson's Bay call this quadruped

B b b 2

Jackash;

Jackass; Mr. Graham from Severn river says, that it harbours about creeks, and lives on fish, like the otter; it travels very slowly, and has from four to seven young at a time; in size it equals the marten; its length is about 16 inches; its whole body is covered with shining dark brown hairs, which lie very close, and seem perfectly convenient for an amphibious animal; under these brown hairs the woolly hairs are tawny, the whole under-jaw is encompassed by a stripe of white hairs, and a little irregular spot of the same colour appears in the middle of the throat; the feet are quite covered with hair to the very nails, which are small, five on each foot, and of a whitish semipellucid colour; the tail is pretty well beset with hair, though not bushy, and much blacker than the rest of the body; it is about half as long as the whole animal.

3. PINE MARTEN. Penn. Syn. Quad. p. 216. n. 155. *Mustela Martes (Abietum)*. Linn.
Severn River. Male and Female.

These seem to be a variety of the yellow-breasted marten, Br. Zool. 1. 81. their colour, especially in the females, being much paler than that described in Mr. Pennant's works. The male is of a chestnut brown, the female a bright tawny yellow; the former has here some dark brown hairs, the latter in the same manner has some bright bay hairs. They both have white cheeks, and white tips of the ears. Their furs are very full of hair, proper

proper to preserve them from the cold. The tail in both sexes is bushy, and darker than the rest of the body; in the female indeed it is tawny, with a black tip; in both it is shorter than described by Mr. Pennant, Mr. Briffon, and others, and was perhaps mutilated. This species feeds on mice, rabbits, &c. though it will not touch a dead mouse which is put as a bait in a trap, and therefore the inhabitants are obliged to make use of a partridge's head, or the like, for that purpose. If pursued with noise, it immediately gets up into a tree. Some gentlemen have unsuccessfully attempted to tame these creatures, and those kept in cages with that view have been observed to be troubled with epileptick fits. Numbers of them are caught at Hudson's Bay in traps made of small sticks. They burrow under ground, and bring forth from four to seven young at a time.

4. STOAT AND ERMINE. Penn. Syn. Quadr. p. 212.
n. 151. *α. β. Mustela Erminca.* Linn.

Severn River, Albany Fort.

One in the summer and another in the winter dress. The natives about Albany call them *Sic-cuse-sue*, but it is not known why they give them that name. They feed on mice, small birds, all sort of fish, flesh, and fowl.

5. COMMON WERBEL. Penn. Syn. Quadr. p. 211.
n. 150. *Mustela mivalis.* Linn.

One in its winter dress, length 7 inches, tail about 1 inch, perhaps mutilated; it is quite white, but
the

the coat is mixed here and there with a brownish hair, especially in the tail. Another in the summer coat, the same as our weasel.

6. SKUNK. Penn. Syn. Quadr. p. 233. n. 167.
Kalm's Travels, l. 273. tab. I.

It answers to Mr. Pennant's description, except that the white stripe on the head is not connected with that on the back, and that the brown area, which is left between the two white stripes on the back, is broader than he describes it.

7. CANADA PORCUPINE. Penn. Syn. Quadr. p. 266.
n. 196. *Hystrix dorsata*. Linn.

Severn River.

It agrees perfectly with the descriptions. These animals live among the pine trees, of which the bark is their food in winter, as willow tops and the like are in summer. They copulate in September, and bring forth only one young the first week in April. During winter they seldom travel above five hundred yards, so that one is always sure of finding a porcupine, as soon as one meets with a tree that has been fresh stripped of its bark. The longest quills of an old porcupine are about five inches long. The Europeans are very fond of the flesh of these animals, as it tastes, when roasted, exactly like that of a sucking pig. Their bones in winter have a greenish yellow colour, perhaps owing to their continually feeding on the bark of pine trees. It is known that

that the bones of animals will become red by their feeding on madder.

8. BEAVER. Penn. Syn. Quadr. p. 255. n. 190.
Castor Fiber. Linn.

Churchill River, N^o 1.

A most beautiful specimen, in high preservation, and in full season; the furr is of a fine jetty black: the skull of another has likewise been sent. There is a great similarity in the conformation of the cutting teeth of this and the preceding quadruped (the porcupine); only the latter has them longer.

9. MUSK-BEAVER. Penn. Syn. Quadr. p. 259. n. 121. *Castor Zibeticus*. Linn.

Musquash. Severn River.

It frequents the plains, builds a house like the beaver, brings forth from five to seven young at a time, and feeds on poplars, willows, and grass.

10. ALPINE HARE. Penn. Syn. Quadr. p. 249. n. 185. *Lepus timidus*. Linn. Kalm's Trav. into N. Amer. III. p. 59.

York Fort.

A fine specimen, in its compleat winter furr, being quite white, except the ears, which have black tips. It is much larger than the following animal. The common hare, *Penn. Syn. Quadr.* does not seem to be a native of America.

11. AMERICAN HARE, called Rabbit at Hudson's Bay. Kalm's Trav. into N. Amer. I. 105. II. 45. Severn and Churchill Rivers.

This species, which has been improperly called Rabbit, perhaps because it is less than the hare, is certainly new, and was never described before, except by Kalm in his travels through North America, Vol. I. 105. II. 45. The account he there gives corresponds with that of Mr. Graham, and with the specimen now in the Royal Society's collection. These animals are numerous at Hudson's Bay; they do not burrow under ground, but live summer and winter under windfalls and roots of trees. They do not migrate, but always keep about the same place, unless disturbed. They breed once or twice a year, and have five to seven young at a time: their weight is from 3 to 4½ pounds. Their flesh is not so white and delicate as that of the common rabbit, but yet is good food in summer and winter. Great numbers of them are annually caught in the following manner: as they always are used to go one particular path, the English and natives lay young trees across it, forming a hedge, in which there is an opening for the creature to go through; in this place they fix a snare, made of brass wire, packthread, or the like, fastened with a slipping knot to a cross piece, the end being tied to an elastic pole; so that when the animal puts its head into

into the snare, the knot is drawn from the cross piece above, and the pole flying up, immediately suspends the animal in the air.

The proper characteristics of this species seem to be,

1. Its size, which is somewhat bigger than a rabbit's, but less than that of the Alpine or lesser hare.
2. The proportion of its limbs, its hind feet being longer in proportion to the body than those of the rabbit and the common hare. Vide the Hon. Daines Barrington's, V.P.R.S. letter to Dr. Watson on this new species of hare, in this volume, p. 6.

3. The tips of the ears and tail, which are constantly grey not black. Kalm's Trav. II, p. 45.

Perhaps some other characters might be ascertained, if the animal was brought over in its perfect summer furr; for all the specimens in the Royal Society's Museum are either entirely in their winter dress, or in a changing condition. Mr. Kalm mentions, that those which are found in New Jersey, where the climate is much more mild than at Hudson's Bay, keep the same grey colour both summer and winter; that in spring they breed in hollow trees, but in summer in the grass; that, when pursued, they immediately take refuge in hollow trees, whence they are driven out by crooked sticks, smoke, &c.; lastly, that they do much mischief to cabbage fields and orchards, by eating the cabbage plants, and

the bark of the apple trees, feeding only by night, as the common hare.

12. QUEBEC MARMOT, Penn. Syn. Quadr. p. 270.
n. 199.

Churchill River, N^o 5.

This creature is called a ground squirrel, at Churchill fort; it differs much in size from that described in the Syn. Quadr. being much less than a rabbit, perhaps it is a young one. I took down the following description, as I did not find it exactly corresponding with that of the Canada marmot. The nose is blunt, the ears are short and roundish, the top of the head chestnut, back all over sprinkled with whitish, black, and yellowish brown: the legs and whole underside of the animal are of a bright ferruginous colour; the tail is very short, and black at the tip. The length of the animal from the nose to the beginning of the tail is about 11 inches, that of the tail 3 inches. Its toes on the fore feet 4; hind feet 5.

13. COMMON SQUIRREL. Penn. Syn. Quadr. p. 279.
n. 206. *Sciurus vulgaris*, Linn.

A variety of the common species, being somewhat inferior in size, having a ferruginous back and grey belly, a shorter tail than the common European sort, of a fine ferruginous red, edged only with black. This animal lives in pine trees, of which the cones are its food; it is dormant the greater part of the winter.

14. GREATER FLYING SQUIRREL.

Severn River.

It is equal in size, if not bigger than the common squirrel; has pretty long hairs, dusky at bottom, tawny brown at the very tips only; and disposed so that the back appears wholly of that reddish brown colour; the tail is very bushy, somewhat compressed, but not pinnated (i. e. with the hairs disposed horizontally on each side of it, as for example in the common squirrel), it is brownish on the upper side with a dusky tip, of a yellowish white below; the whole underside of the animal has the same yellowish white colour. The membrane reaches from the forefeet to the hindfeet, without extending to the ears; it is found in James's Bay, about 51° north latitude.

This is perhaps Linneus's *Sciurus volans*, and the same with the flying squirrel of the Arctick parts of Europe. Mr. Brisson seems to have confounded this, and the little Virginian squirrel together, and his quotations are quite confused. Linneus's *Mus volans* certainly is a variety of the little flying squirrel, of the milder parts of North America, New York, Pennsylvania, Virginia, which is vastly different from this in size and colour.

15. A SMALL ANIMAL, called a Field Mouse.

Churchill River.

A specimen in very bad preservation, wanting legs, tail, &c which makes it impossible to de-

termine of what species it is ; its size is somewhat superior to that of a mouse, its colour dusky, mixed with tawny brown, and dirty white on the belly ; its head is broad, like that of the short-tailed field mouse, and has a dusky line in the middle between the eyes, which extends, though rather indistinctly, all along the back ; its ears are very small and roundish.

16.

This is likewise a very bad mutilated specimen, less than the common mouse, dusky and brown above, and whitish below ; its ears are pretty large and prominent.

17. FIELD MOUSE. Penn. Syn. Quadr. p. 302. n. 230. *Mus Sylvaticus*, Linn.

Two specimens ; the descriptions answer pretty well, the ears are large and round, the tail is very long and whitish below.

18. SHORT-TAILED MOUSE. Penn. Syn. Quadr. p. 305. n. 233. *Mus terrestris*, Linn. Le Campagnol de Buffon.

Mr. Pennant's admeasurements do not quite answer, but M. d'Aubenton's coincide.

19. FOETID SHREW. Penn. Syn. Quadr. p. 307. n. 235. *Sorex Araneus*, Linn.

The specimen is much blacker on the back than the European Shrew, its sides are reddish brown.

20. SHREW.

20. SHREW; two specimens.

The colour is of a dusky grey above, and a dirty white or yellowish below; the nose is very long and slender; the length from the nose to the tail, in the one specimen is 2½, in the other almost 2 inches; the tail is about an inch and half long, thinly beset with hairs, brown above, and yellowish below. If this species had no tail, I should take it to be the minute Shrew, which the Rev. Mr. Laxman found in Siberia, and which is the *Sorex minutus*. Linn.

XXIX. *An Account of the Birds sent from Hudson's Bay; with Observations relative to their Natural History; and Latin Descriptions of some of the most uncommon.*
By J. R. Forster, F. R. S.

Read June 18—25, 1772.

I. LAND-BIRDS.

1. { Accipitres
{ Rapacious. Faun. Am. Sept.

1. FALCO, } 1. Columbarius. 128. 21. Pigeon Hawk.
Falcon. } Faun. Am. Sept. p. 9. Catesby I. t. 3.
Epervier de la Caroline. Brisson I. p. 378.
Severn river, N° 19.

This species is called a *small-bird hawk* at Hudson's Bay. It is migratory, arriving near Severn River in May, breeding on the coast, and then retiring to a warmer climate in autumn. It feeds on small birds; and, on the approach of any person, will fly in circles, making a hideous shrieking noise. The breast and

and belly are yellowish, with brown streaks, which are not mentioned by the ornithologists, though their descriptions answer in other respects. It weighs six ounces and a half, its length is $10\frac{1}{2}$, the breadth $22\frac{1}{2}$. Catesby's figure is a very indifferent one.

FALCO, 2. Spadiceus. *New Species*. Chocolate Falcon. Faun. Am. Sept. p. 9.

This species, at first sight, bears some resemblance to the European Moor Buzzard, or *Aeruginosus*, Linn. but is much less, and wants the light spots on the head and shoulders. No number or description was sent along with it.

FALCO, 3. Sacer, Brisson, I. p. 337. Sacre de Buffon, Oiseaux, (edition in 12mo.) Tom. II. p. 349. t. 14. Faun. Am. Sept. p. 9. Severn River; N° 16.

Speckled Partridge Hawk, at Hudson's Bay. The name is derived from its feeding on the birds of the Grouse tribe, commonly called partridges, at Hudson's Bay. Its irides are yellow, and the legs blue. It comes nearest the *Sacre* of Brisson, Buffon, and Belon; but Buffon says it has black eyes, which is very indistinct; for the irides are black in none of the falcons, and in few other birds; and the pupil, if he means that, is black in all birds. It is said, by Belon, to come from Tartary and Russia, and is, therefore, probably a northern bird. It is very voracious
and

and bold, catching partridges out of a covey, which the Europeans are driving into their nests. It breeds in April and May. Its young are ready to fly in the middle of June. Its nests, as those of all other falcons, are built in unfrequented places; therefore, the author of the account from Severn River could not ascertain how many eggs it lays; however, the Indians told him it commonly lay two. It never migrates, and weighs 2½ pounds; its length is 22 inches, its breadth 3 feet.

2. STAIR. 14. *Bubo virginianus*. The Short-eared Owl. Owl. Brit. Zoology, folio, plate B. 3. octavo, I. p. 156. Faun. Am. Sept. 9. Severn River, N° 17 and 64.

Moose Hawk at Hudson's Bay. It answers the description and figure in the British Zoology; but its ears or long feathers do not appear. The smallness of the head has, probably, given occasion to call it a hawk, though it does not fly about in quest of prey, like other hawks (as the account from Severn River says); it sits quiet on the stumps of trees, waiting mice with all the attention of a domestic cat, being an insatiable enemy of those little animals. It migrates southward in winter, and breeds along the coast, its irides are yellow; its weight is 14 ounces; its length 16 inches, its breadth 3 feet.

STAIR. 14. *Bubo virginianus*. The Short-eared Owl. Owl. Brit. Zoology, folio, plate B. 3. octavo, I. p. 156. Faun. Am. Sept. 9. Severn River, N° 17 and 64.

STRIX, 5. *Nyctea*. 132. 6. Snowy Owl. Faun.
Am. Sept. 9.

Churchill River, N° 7. White Owl.

It seems to be in its winter dress, as it is intirely white. The feet are covered with long white hair-like feathers to the very nails, but there are none on the soles or under parts of the toes.

STRIX, 6. *Funerea*, 133. 11. Canada Owl. Faun.
Am. Sept. 9.

Severn River, N° 13. Churchill River, N° 11.

Cabeticuch, or *Cabaducutch*, is the Indian name of this bird. Linneus's description answers perfectly. The male, which in the class of birds of prey is generally smaller, is, however, in this species, larger than the female, according to the account from Severn River. Its colour is likewise much blacker, and the spots more distinct. The eyes are large and prominent, the irides of a bright yellow. The weight is 12 ounces; its length 17 inches, the breadth 2 feet. It has only two young at one hatching.

STRIX, 7. *Passerina*. 133. 12. Little Owl. Britz.
Zool. Faun. Am. Sept. 9.

(The number belonging to this bird is lost, but it is most probably that from Severn River, N° 15. called *Shipompsish* by the natives).

The crown of the head is speckled with white, as in the *Strix funerea*.

STRIX, 8. *Nebulosa*. *New species*. The grey Owl.
Severn River, N° 36.

This fine non-descript owl lives upon hares, ptarmigans, mice, &c. It has two young at a time. The specimen sent over is said to be one of the largest. It is not described by any author. Its weight is 3 pounds, length 16 inches, breadth 4 feet.

3. LANIUS, } 9. *Excubitor*. 135. 11. Great Butcher-Shrike. } bird. Brit. Zool. Cinereous Shrike.
Faun. Am. Sept.

Severn River, N° 11.

White Whiskjahn at Hudson's Bay. The specimen is a male; it weighs two ounces and a half, is seldom found on the coast, but frequent about a hundred miles inland; and feeds on small birds. It corresponds with ours in every respect.

II. { *Picæ*.
 { *Pics*. Faun. Am. Sept.

4. CORVUS, } 10. *Canadensis*. 158. 16. Cinereous Crow. } Crow. Faun. Am. Sept. 9.
Severn River, N° 9 and 10.

These birds are called *Whiskjahn* and *Whiskjack* at the Hudson's Bay. They weigh 2 ounces; and are 9 inches long, and 11 broad. Their eyes are black, and their feet of the same colour. Their characters correspond with the Linnæan description. They breed early in spring; their nests are made of sticks and grass.

grafs, and built in pine trees; they have two, rarely three, young ones at a time; their eggs are blue; they fly in pairs; the male and female are perfectly alike; they feed on black moss, worms, and even flesh. When near habitations or tents, they are apt to pilfer every thing they can come at, even salt meat; they are bold, and come into the tents to eat victuals out of the dishes. They watch persons baiting the traps for martins, and devour the bait as soon as they turn their backs. These birds lay up stores for the winter, and are seldom seen in January, unless near habitations; they are a kind of mock-bird; when caught, they pine away and die, though their appetite never fails them.

CORVUS, 11. Pica. 157. 13. Magpie. Brit. Zool. Faun. Am. Sept. 9.

Albany Fort, N° 5.

It is called *Oue-ta-kee aske*, i. e. *Heart-bird*, by the Indians. It is a bird of passage, and rarely seen; it agrees, in all respects, with the European magpie, upon comparison.

5. PICUS, } 12. Auratus. 174. 9. Gold-wing
Woodpecker. } Woodpecker. Faun. Am. Sept. 10.
Catelby, I. 18.

Albany Fort, N° 4. the large Woodpecker.

The natives of America call this bird *Ou-thee-guan-nor-nor*, from the yellow colour of the shafts of the quill and underside of the tail feathers. It is a bird of passage; visits the

neighbourhood of Albany Fort in April, leaves it in September ; lays from four to six eggs in hollow trees, feeds on small worms and other insects. Its descriptions answer exactly.

Picus, 13. Villosus, 175. 16. Hairy Woodpecker.
Faun. Am. Sept. 10. Catesby I. 19.

Severn River, N° 56.

The specimen sent over is a female, by its wanting the red on the head. The descriptions of Linneus and Brisson agree; only the two middlemost feathers are black, the next are of the same colour, but have a white rhomboidal spot near the tip; the next are black, with the upper half obliquely white, the very tip being black; the next after that are white, with a round black spot on the inner side close to the base, and the lower part of the shaft is black, the outermost feathers are quite white, the shaft only at the base being black.

14. Tridaëtylus. 177. 21. Three-toed Woodpecker.
Faun. Am. Sept.

Severn River, N° 8.

A female, weight 2 ounces, length 8 inches, breadth 13; eyes dark blue, legs black. It builds its nest in trees, lives in woods upon worms picked out of trees, is not very common at Severn River. The descriptions answer.

III. { Gallinæ.
Gallinaceous. Faun. Am. Sept.

6. Tetrao. { 15 Canadensis, 274. 3. } Faun. Am. Sept. 10.
Grouse. { Canace, 275. 7. } Spotted Grouse.
Gelinotte du Canada, male et femelle, Pl. enl.
131 et 132. Buffon Oiseaux II. p. 279. 4to.
Briffon I. p. 203. t. 20. f. 1, 2, and p. 201. app.
10. Edwards, t. 118 and 71.

Severn River, N° 5. Woodpartridge.

These birds are all the year long at Hudson's Bay, and never change the colour of their plumage. The accounts from Hudson's Bay say, there is no material difference between the male and female; which must be a mistake, as they are really very different. Linneus's descriptions of the Tetrao Canadensis, and Canace, both answer to the specimens sent over, so that, after comparing them, I find they are only one and the same species. I suppose the dividing them into two, was occasioned by Briffon's and Edwards's descriptions, being taken from specimens sent from different parts of the continent of America, and perhaps caught at different seasons. Mr. de Buffon has, I find, the same opinion with me, and by comparing the drawings of Edwards, with those of the Planches enluminées, it is put beyond a doubt. These birds are very stupid, may be knocked down with a stick, and are frequently caught by the natives.

tives with a stick and a loop. In summer they are good eating; but in winter they taste strongly of the pine spruce, upon which they feed during that season, eating berries in summer. They live in pine woods, their nests are on the ground; they generally lay but five eggs.

Tetrao, 16. Lagopus, 274. 4. White Grouse. Faun. Am. Sept. 10. Ptarmigan. Br. Zool. Lagopède de la Baye de Hudson. Buffon Oiseaux II. p. 276. Edw. t. 72.

Severn River. N^o 1—4. Willow-partridges.

The Hudson's Bay ptarmigan has been separated from the European in the British Zoology, and afterwards by M. de Buffon: however, I must own, I cannot yet find the differences which they assign to these species. They contend that the Hudson's Bay bird figured by Edwards is twice as big as the European ptarmigan; Mr. Edwards, I think, does not intimate this, when he says, the bird is of a middle size, between partridge and pheasant; he on the contrary supposes them to be the same species. The British Zoology, after Willoughby, says, the ptarmigan's length is 13½ inches. The account from Severn River says it is 16½ inches. The breadth in the British Zoology is said to be 2½ inches. The breadth in the Hudson's Bay birds, according to the accounts from Severn River, is 2½ inches. Willoughby's ptarmigan weighed 14 ounces; that in the British Zool.

Zool. illustr. t. 13. 19 ounces; that from the Hudson's Bay ($1\frac{1}{2}$ lb) 24 ounces. These differences are of little consequence, and far from increasing the Hudson's Bay bird to double the size of the European. The British Zoology says, there is a difference in the summer colours; but Mr. Edwards informs us, that he compared the Hudson's Bay bird with the descriptions of former ornithologists, and found them to answer; he likewise assures us he had the same bird from Norway. Therefore I cannot help dissenting from the British Zoology, in this one particular, and thinking with Linneus and Brisson, that the European and Hudson's Bay ptarmigans are the same, especially as the colours vary very much in the different sexes and at different seasons. To this we may add the testimony of a gentleman well versed in natural history, who, having had opportunities of comparing numbers of Hudson's Bay and European ptarmigans, assured me that he did not see any difference between them. They go together in great flocks in the beginning of October, living among the willows, of which they eat the tops (whence they have got the name of willow partridges): about that time they lose their beautiful summer plumage, and exchange it for a snowy white dress, most providently adapted by its thicknets to screen them against the severity of the season, and by its colour against their enemies

the

the hawk and owl, against whose attacks they would otherwise find no shelter. Each feather is double, that is, a short one under a long one, to keep them warm. In the latter end of March, they begin again to change their plumage, and have got their full summer drets by the end of June. They breed every where along the coast, and have from nine to eleven young at a time; making their nests on the ground, generally on dry ridges. They are excellent eating, and so plentiful that ten thousand have been taken at Severn, York, and Churchill Forts. The method of netting or catching them, is as follows: a net made of jack-twine, twenty feet square, is laced to four long poles, and supported in front with the sticks, in a perpendicular situation; a long line is fastened to these supports, one end of it reaching to a place where a person lies concealed; several men drive the ptarmigans (which are as tame as chickens, especially on a mild, snowy day), towards the net, which they run to, as soon as they see it. The person concealed draws the line, by which means the net falls down, and catches 50 or 70 ptarmigans at once. They are sometimes rather wild, but grow better humoured (as Mr. Graham says) by being driven about, for they seldom forsake those willows which they have once frequented.

TETRAO. 17. *Togatus*, 275. 8. Shoulder-knot
Grouse. *Grosse Gelinotte du Canada*. Pl. enl. 104.
Briss. I. 207. t. 21. f. 1. Buffon Oiseaux II. p.
287.

Severn River, N° 60 and 61. Albany Fort 1 and 2.

This bird answers the descriptions given of it by the ornithologists in all respects, and perfectly resembles the figure in Brissón, and in the Planches enluminées. It differs from Edwards's ruffed heathcock, t. 248. or Linneus's *Tetrao umbellus*, as the latter has not the shining black axillar feathers, or shoulder-knot, but a ferruginous one, is much less, and has brighter colours. M. de Buffon, however, thinks they are the same, and suspects at the same time, that the bird which he calls *la grosse Gelinotte du Canada* (and which is the same with the Society's specimens) is the female of Mr. Edwards's bird, t. 248. This conjecture is destroyed by the specimens now sent from Hudson's Bay, which by the accounts from thence are expressly said to be males. The shoulder-knot grouses bear the Indian name of *Puskee*, or *Puspuskee*, at Hudson's Bay, on account of the leanness and dryness of their flesh, which is extremely white, and of a very close texture, but when well prepared is excellent eating. They are pretty common at Moose Port and Henly House, but are seldom seen at Albany Fort, or to the northward of the above places. In winter they feed upon jun-

niper tops, in summer on goose-berries, raspberries, currants, cranberries, &c. They are not migratory, staying all the year at Moose Port; they build their nests on dry ground, hatch nine young at a time, to which the mother clucks, as our common hen does; and on the least appearance of danger, or in order to enjoy a comfortable degree of warmth, the young ones retire under the wings of their parent.

N. B. A specimen, which is supposed to be either a young bird or a female, wants the blueish black shoulder-knot; but it is the same in all other respects.

TETRAO, 18. *Phasianellus*. Linn. Syst. Nat. Ed. X. p. 160. n. 5. Edw. 117. Longtailed Grouse. Faun. Am. Septentr. 10.

Severn River, N° 6 and 7. Albany Fort, N° 3.

This bird, which Mr. Edwards has drawn plate 117, was by Linneus in the tenth edition of his System, ranged as a new species of grouse or tetrao, by the specific name of *Phasianellus* (alluding to the name of Pheasant which it bears at Hudson's Bay, and likewise to its pointed tail). He afterwards in the new or twelfth edition of the System, p. 273. makes it a variety of the great Cock of the Wood, or Tetrao *Urogallus*, probably from the account in Mr. Edwards, that the male struts very upright, is in general of a darker colour than the female, and has a glossy neck. These circumstances, however, are not sufficient to bring;

bring them under the same species, for it is known that the males of all the grouse tribe, and indeed of most of the gallinaceous birds, are used to strut in a very stately manner, and that the colours of their plumage are much more distinct than those of the females. But the specific difference alone, which Linneus assigns to the cock of the wood, absolutely excludes our Hudson's Bay species; he calls it *Tetrao pedibus hirsutis, cauda rotundata, axillis albis*. Whoever examines Mr. Edwards's figure, and the specimens now in the Society's possession, will find the tail very short, but pointed, the two middle feathers being half an inch longer than the rest, (Mr. Edwards says two inches) and the axillæ, or shoulders, by no means white: besides this difference, the colour and size of the Hudson's Bay bird are likewise vastly different from those of the cock of the wood. Its length is 17 inches, its breadth 24, and, as Mr. Edwards justly says, it is somewhat bigger than the common pheasant. The great cock of the wood is as big as a turkey; and its female, which is much less, however far exceeds our bird, it being 26 inches long, and 40 broad. See *British Zool.* octavo, p. 200. The figures given of the female of the *T. Urogallus*, or great cock of the wood, in the *Br. Zool. folio*, plate M*, and the *Planche enluminee* 75, will serve upon comparison as a convincing proof of the vast difference there is between the Hudson's Bay pheasant grouse and the European cock

of the wood. The figure, which Mr. Edwards has given of the former bird, does not exactly correspond with the Society's specimen, as he has represented the marks on the breast half-moon shaped, though they are heart-shaped as those on the belly in the dried bird; that is, they are white spots, with a pale brownish yellow cordated brim. Nor can I agree with Mr. Edwards, when he calls this bird the long-tailed grouse from Hudson's Bay; for its tail is really very short, in comparison with that of other grouse, and its smallness and acuteness afford one of the most distinguishing characters of the species.

The native Indians call these pheasant grouses, *Oc-kist-cow*: they are found all the year long, amongst the small juniper bushes, of which the buds are their principal food, as also the buds of birch in winter, and all sorts of berries in summer. They never vary their colours; nor is there any great difference between the male and female, except in the caruncula or comb over the eye, which in the male is an inch long, and $\frac{1}{4}$ of an inch high. The account from Albany Fort adds, that the colour of the male is somewhat browner, and almost a chocolate on the breast. Their flesh is of a light brown, exceeding juicy, and they are very plump. They lay from 9 to 13 eggs; their young can run almost as soon as they are hatched; they make a piping noise somewhat like a chicken. The cock has a shrill crowing note, not very loud;

but

but when disturbed, or whilst flying, he makes a repeated noise of cuck, cock. They are most common in winter at Albany Fort.

Before I leave the genus of grouses, I must observe that their feet have a peculiarity, taken notice of by few authors; the toes, in several species, have on each side a row of short flexible teeth, like those of a comb; so that the toes appear pectinated. The species, which are known to have such pectinated toes, are,

1. The great Cock of the Wood, *Tetrao. Urogallus*, Linn.
2. The Black Cock, *T. Tetrix*, Linn.
3. The Spotted Grouse, { *T. Canadensis*,
and { *T. Canace*, Linn..
4. The Ruffed Grouse, *T. Umbellus*, Linn..
5. The Shoulder-knot Grouse, *T. Togatus*, Linn.
- 6 The Pheasant Grouse, *T. Phasianellus*.
7. The Hazel Hen, *T. Bonasia*, Linn.
8. The Pyrenæan Grouse, *T. Alchata*, Linn..

This is a circumstance, which ought to be attended to in all other species of grouses, as it may in time afford a distinguishing character for a division in this great genus; the ptarmigan, or *T. Lagopus*, Linn. is without these teeth.

IV. { Columba.
 { Columbine. Faun. Am. Sept.

7. COLUMBA, } 19. Migratoria. 285. 36. Migratory
 Pigeon. } Pigeon. Catsh. I. 23. Kalin II.
 p. 82, t. Passenger Pigeon, Faun. Am. Sept. 11.
 Severn River, N° 63. Wood-pigeon.

These pigeons are very scarce so far northward as Severn river, but abound near Moose-fort, and further inland to the southward. Their common food are berries and juniper buds in winter; they fly about in great flocks, and are reckoned good eating. This account is confirmed by Kalin in his travels (English edition) Vol. II. p. 82 and 311. They hatch only two eggs at a time, and their nests are built in trees. Their eyes are small and black, the irides yellow, the feet red: the neck finely glossed with purple, brighter in the male. They weigh 9 ounces.

V. { Passeres.
 { Passerine. Faun. Am. Sept.

8. Alauda. } 20. Alpestris. 289. 10. Klein, Hist. of
 Lark. } Birds, 4to. p. 73. Shore Lark, Faun.
 Am. Sept. 12. Catsh. I. 32.
 Albany Fort, N° 6.

This species is indifferently described by Linnæus, who says that all the tail-feathers on their inner web are white, (*rectricibus dimidio interiore albis*); though it does not appear that he saw a specimen of it himself. Both the
 quill

quill and tail-feathers are dusky, and in both the outermost feather only has a white exterior margin. The coverts of the tail are of a pale ferruginous colour, and two of them are nearly as long as the tail itself. The scapulars are ferruginous; in the male, the head and whole back have a tinge of the same colour, marked with dusky streaks; in the female, the back is grey, and the dusky stripes of a darker hue. The crown of the head is black in the male, dusky in the female; the forehead is yellow, the bill and feet are black, the belly of a dirty reddish white. These larks are migratory, they visit the environs of Albany Fort in the beginning of May, but go further northward to breed: they feed on grass-seeds, and buds of the sprig-birch; run into small holes, and keep close to the ground, from whence the natives give them the name of *Cbi-chup-pi-sue*.

9. Turdus. } 21. Migratorius, 292. 6. American
Thrush. } Fieldfare. Kalm II. p. 90. Faun. Am.
Sept. II. Catesby I. 29.

Severn River, N° 59. Albany Fort, 7, 8, 9.

The descriptions of these birds in various authors coincide with the specimens; at Severn River they appear at the beginning of May, and leave the environs before the frost sets in. At Moose Fort, in the north latitude 51°. they build their nest, lay their eggs, and hatch their young in the space of fourteen days; but at York fort and Severn settlement this is done.

done in 25 days: they build their nests in trees, lay four beautiful light-blue eggs, feed on worms and carrion: when at liberty they sing very prettily, but confined in a cage, they lose their melody. There is no material distinction between the male and female. Their weight is 21 ounces, the length 9 inches, and the breadth 1 foot; they are called red birds at Hudson's Bay; their Indian name is *Pee-pee-chue*.

Turdus, 22.

Severn River, N^o 54 and 55, male and female.

From the striking similarity with our blackbird, the English at Hudson's Bay have given this bird the same name. However, upon a close examination, I find the difference very great between our European blackbird, and the Hudson's Bay or American one. The plumage of the male, instead of being deep black without any gloss, as in ours, has a shining purple cast, not unlike the plumage of the *Gracula Quiscalus*, Linn. or shining Grackle, Paun. Am. Sept.; or the Maize thief, of Kalm. The female indeed is very like our female blackbird, being of a dusky colour on the back, and a dark grey on the breast. The feet and bill are quite black in both sexes; the former have the back claw almost as long again as any of the other claws. There are no vestiges of yellow palpebrae in either the male or the female; the bill in both is strong, smooth, and subulated; the upper

upper mandible being carinated, but very little arched, and without any tooth or indentation whatever, on the lower side. The nostrils are as in other thrushes. This bird has no bristles at the base of its bill, its feet have such segments as Scopoli in the *Annus I. Historico-Naturalis* attributes to the *stares*. Instead of being solitary and living retired like the European blackbirds, these American ones come in flocks to Severn River in June, live among the willows, build in all kinds of trees, and return to the southward in autumn. They feed on worms and maggots; their weight is 2½ ounces, and they are nine inches long, and one foot broad. One that was kept twelve months in a cage pined away, and died. Notwithstanding these circumstances, I cannot help remaining undetermined with regard to this bird, which at first sight is like the blackbird, has the bill of a thrush, and the feet and gregarious nature of a *stare*. It is to be hoped, that future accounts from Hudson's Bay may inform us further, of the nature of this bird, its time of incubation, the number of eggs it lays, and the colour of those eggs, together with the note of the bird, the difference and characteristick marks of both the male, and female, and other circumstances, which may serve to determine to what genus and species we are to refer this bird.

10. Loxia, { 23. Curvirostra, 299. 1. Crossbill.
Grosbeak. { Br. Zool. Faun. Am. Sept. 11. The
small variety.

Severn River, N^o 27 and 28.

This bird comes to Severn River the latter end of May, breeds more to the northward, and returns in autumn, in its way to the south, departing at the setting in of the frost. The irides in the male are of a beautiful red, in the female yellow: the weight is said to be 10 ounces (probably by mistake for 1 ounce, as it is impossible so small a bird should weigh more), the length is 6 inches, the breadth 10.

24. Euculeator, 299. 3. Pine Grosbeak. Br. Zool. and Faun. Am. Sept. Edw. 123, 124. Pl. enl. 135. f. 1.

Severn River, N^o 29, 30.

It answers to the descriptions and figures of the ornithologists pretty well; only Edwards's female has the red too bright, which is rather orange in our specimen, on the head, neck, and coverts of the tail. This bird only visits the Hudson's Bay settlements in May, on its way to the north, and is not observed to return in autumn; its food consists of birch-willow buds, and others of the same nature, it weighs 2 ounces, is 9 inches long, and 23 broad.

11. EMBERIZA. { 25. Nivalis. 308. 1. Greater
Bunting. { Brambling, Br. Zool. Snowbird
Snowflake, ibid. Snow-bunting. Faun. Am. Sept.

11.

Severn River, N° 24—26.

The bird, in summer dress, corresponds exactly with the description of the greater brambling, Br. Zool. The description of the snowflake, or the same bird in winter dress, ibid. vol. IV p. 19. is somewhat different, perhaps owing to the different seasons the birds were caught in, as it is well known they change their colour gradually. They are the first of the migratory birds, which come in spring to Severn settlement; in the year 1771 they appeared April the 11th, stayed about a month or five weeks, and then proceeded further northward in order to breed there; they return in September, stay till the cold grows severe in November, then retire southward to a warmer climate. They live in flocks, feed on grass-seeds, and about the dunghills, are easily caught under a small net, some oatmeal being strewed under it to allure them; they are very fat, and fine eating. The weight is 1 ounce and 5 drams, the length 6½ inches, and the breadth 10 inches.

EM BRIZA. 26. Leucophrys. *New Species*. White
Crowned Bunting.

Severn River, N° 50. Albany Fort, 10.

This elegant little species of Bunting is called
a hedge sparrow at Hudson's Bay, and has

not hitherto been described. It visits Severn settlement in June, and feeds on grass-seeds, little worms, grubs, &c. It weighs $\frac{1}{4}$ of an ounce, and is 7 $\frac{1}{2}$ inches long, and 9 inches broad; the bill and legs are flesh-coloured; the male is not materially different from the female, its nests are built in the bottom of willow bushes, it lays three eggs of a chocolate colour. It visits Albany Fort in May, breeds there, and leaves it in September.

12. FRINGILLA, { 27. Lapponica. 317. 1. Faun.
Finch. { Succ. 235.

Severn river, N^o 52.

It is called *Tecurmasbisk*, by the natives at Hudson's Bay. The description in Linneus's Fauna Suecica coincides exactly with the specimen; that in his System answers very nearly: Mr. Brisson's description (though he quotes Linneus, and Linneus quotes him) is widely different. The specimen sent over is a female; the males have more of the ferruginous colour on the head; the eyes are blue, the legs dark brown. It is only a winter inhabitant near Severn river, appears not before November, and is commonly found among the juniper trees; it weighs $\frac{1}{4}$ of an ounce, its length is 5 inches, and its breadth 7.

FRINGILLA.

L'RINGILLA. 28. Linaria. 322. 29. Lesser red headed Linnet. Br. Zool.

Severn River, N° 23.

The descriptions of Linneus, Brisson, and the British Zoology, answer perfectly well. The figure in Planche enluminée 151. f. 2. has a quite ferruginous back contrary to all the descriptions and the specimen before us, in which all the feathers on the back are dusky, edged with dirty white.

29. Montana, 324. 37. Mountain Sparrow, Tree Sparrow. Br. Zool. Edw. 269. Brisson III. p.

79. Faun. Am. Sept.

Severn River, N° 20.

This seems to be a variety, as its tail is rather longer than usual, and forked; it answers nearly to the descriptions given by the ornithologists, and seems to be a female, as it has no black under the throat and eyes, and no white collar. The bill and legs are black, the eyes blue. At Severn settlement it arrives in May, goes to breed further northwards, and returns in autumn: the weight is $\frac{1}{2}$ of an ounce, the length $6\frac{1}{2}$ inches, and breadth 10. I was inclined to make this bird a new species, on account of the many differences between it and the mountain sparrow; but considering the specimen sent over was not in the best order, and might be a female, I thought it best to leave it where it is, till we are better informed.

FRIN-

FRINGILLA. 30. *Hudsonias.* *New Specimen.*
 Severn River, N^o 18.

This is certainly a nondescript species; it only visits Severn settlement in summer, not being seen there before June, when it stays about a fortnight, goes further to the northward to breed, and passes by Severn again in autumn on its return south. It is very difficult to procure, and therefore it could not be determined whether the specimen was a male or female. It frequents the plains, and lives on grass-seeds; it weighs $\frac{1}{2}$ an ounce, is 6 $\frac{1}{2}$ inches long, and 9 inches broad: it has a small blue eye, and a whitish bill faintly tinged with red; the whole body is blackish, or of a foot colour, the belly alone with the two outermost tail feathers on each side being white. It is to be wished that more specimens and circumstantial accounts of this bird were sent over, which would enable us to determine its character with more precision.

13. **MUSCICAPA.** 31. *Striata.* *New Species.* Striped Flycatcher. { Flycatcher.

Severn River, N^o 48 and 49. Male and Female.

This species visits Severn river only in summer, feeding on grass-seeds, etc.; it weighs half an ounce, is 5 inches long, and seven broad; the male is widely different from the female: this species is entirely nondescript.

14. MOTACILLA, { 32. Calendula. 337. 47. Ruby
Wagtail. { crowned Wren. Edw. 254.
Faun. Am. Sept.

(The number belonging to this bird is lost; however, it is most probably that sent from Severn river, N^o 53.)

It answers to the descriptions and the figure of Edwards; its weight is 4 drams, its length 4 inches, and its breath 5. It migrates, feeds on grass-seeds and the like, and breeds in the plains; the number of eggs is not known.

15. PARUS, { 33. Atricapillus. 341. 6. Black Cap.
Titmouse. { Titmouse.
Albany Fort, N^o 11.

The description given by Linneus answers, and so does M. Brisson's in most particulars, except that the quill-feathers are not white on the inside. These birds stay at Albany Fort all the year, yet seem most numerous in the coldest weather; probably being then more in want of food, they come nearer the settlements, in order to pick up all remnants. They feed on flies and small maggots, and likewise on the buds of the sprig-birch, in which they perhaps only search for insects; they make a twittering noise, from which the native call them *Kiss-kiss-ke-shish*.

PARUS. 34. Hudsonicus. *New Species.* Hudson's Bay Titmouse.

Severn River, N° 12.

This new species of titmouse, is called *Peeche-ke-ke-flish*, by the natives. They are common about the juniper bushes, of which the buds are their food; in winter they fly about from tree to tree in small flocks, the severest weather not excepted. They breed about the settlements, and lay 5 eggs; they have small eyes, with a white streak under them, and black legs: the male and female are quite alike; they weigh half an ounce, are $5\frac{1}{2}$ inches long, and 7 inches broad.

16. HIRUNDO, } 35.

Swallow. }

Severn River, N° 58.

The swallows build under the windows, and on the face of steep banks of the river, they disappear in autumn; and the Indians say, they were never found torpid under water, probably because they have no large nets to fish with under the ice. The specimen sent answers in some particulars to the description of the Martin, *Hirundo Urbica*, Linn. but seems to be smaller, and has no white on the rump. I have, therefore, thought it best to leave the species undetermined, till further informations are received from Hudson's Bay, on this subject.

2. WATER-BIRDS.

VI. { GRALLÆ,
Clovenfooted. Faun. Am. Sept.

17. ARDEA, { 36. Canadensis. 234. 3. Edw. 133.
Heron. { Canada Crane. Faun. Am. Sept. 14.
Severn River, N° 35. Blue Crane.

The account from Severn settlement says, there is no material difference between the male and female; however, the specimen sent over, I take to be a female, as its plumage is in general duller than that figured by Edwards, and as the last row of white coverts of the wing are wanting. These cranes arrive near Severn in May, have only two young at a time, retire southward in autumn; frequent lakes and ponds, and feed on fish, worms, &c. They weigh seven pounds and a half, are 3½ feet long, and 3 feet 5 inches broad; the bill is 4 inches long, the legs 7 inches, but the leg and thigh 19.

ARDEA. 37. Americana, 234. 5. Hooping Crane.
Edw. 132. Catesby, l. 75. Faun. Am. Sept.

14.
York Fort.

Edwards's figure is very exact; Catesby's is not so good, as it represents the bill too thick towards the point.

38. *Stellaris*, 239. 21. *Varietas*. The Bittern, Br. Zool. Edw. 136. Faun. Am. Sept. pag. 14. Severn River, N^o 64.

At first sight, I thought the specimen sent from Hudson's Bay, was a young bird; but upon nearer examination and comparing it with Mr. Edwards's account and figure, I take it to be a variety of the common bittern peculiar to North America; it is smaller, but upon the whole very much resembles our bittern. Mr. Edwards's measurements and drawings correspond very well with the specimen.

This bird appears at Severn river the latter end of May, lives chiefly among the swamps and willows, where it builds its nest, and lays only two eggs at a time; it is very indolent, and, when roused, removes only to a short distance.

18. *SCOLOPAX*, { 39. *Totanus*. 245. 12. Spotted Woodcock. } Woodcock. Faun. Am. Sept. 14. Albany Fort, N^o 16.

This bird is called a yellow leg at Albany fort, from the bright yellow colour of the legs, especially in old birds; a circumstance, in which it varies from the descriptions of Linneus and Brisson, probably because they de-

* In the Faunula America Septentrionalis, p. 14. the synonym of *Ardea Hudsoniae*, Linn. has by mistake been annexed to the bittern, and likewise pl. 135 of Edwards has been quoted instead of plate 136. They are two very different birds.

scribed from dried specimens, in which the yellow colour always changes into brown. It agrees in other respects perfectly well with the descriptions: it comes to Albany fort in April or beginning of May, and leaves it the latter end of September. It feeds on small shell fish, worms, and maggots; and frequents the banks of rivers, swamps, &c. It is called by the natives *Sa-sa-she-w*, from the noise it makes.

SCOLOPAX. 40. *Lapponica*. 246. 15. Red Godwit. Br. Zool. Faun. Am. Sept. 14. Ed. 138. Churchill River, N° 13.

Linneus describes this bird very exactly in his *Systema Naturæ*: the middle of the belly has no white in the Society's specimen, as that had from which the description in the Br. Zool. octavo I. p. 353, 354, was taken. All the other characters correspond.

SCOLOPAX. 41. *Borealis*. *New Species*. Eskimaux Curlew. Faun. Am. Sept. 14. Albany Fort, N° 15.

This species of Curlew, is not yet known to the ornithologists; the first mention is made of it in the *Faunula Americæ Septentrionalis*, or catalogue of North American animals. It is called *Wee-kee-me-nase-su*, by the natives; feeds on swamps, worms, grubs, &c; visits Albany Fort in April or beginning of May; breeds to the northward of it, returns in Au-

gust, and goes away southward again the latter end of September.

19. TRINGA, { 42. Interpres. 248. 4. Turnstone.
Sand-piper. { Edw. 141. Faun. Am. Sept. 14.
Severn River, N^o 31 and 32.

This species is well described by the ornithologists; its weight is 3½ ounces, the length 8½ inches, and the breadth 17 inches; it has four young at a time; its eyes are black, and the feet of a bright orange: this bird frequents the sides of the river.

43. Helvetica. 250. 12, Brisson. Av. V. p. 106.
t. 10. f. 2.

(The number was lost, perhaps it is N^o 17, from Fort Albany; upon that supposition the account is as follows: "the natives call it "*Waw-pusk-abrea-shish*, or white bear bird; "it feeds on berries, insects, grubs, worms, "and small shell-fish; visits and leaves Albany fort at the same time with the *Sco-* "*lopax Totanus*, and *Borealis*.")

I find this bird answers very well to its description; the throat, breast, and upper part of the belly are blackish, as in the descriptions, but mixed with white lunulated spots, which are neither described nor expressed in M. Brisson's figure, and may be owing to the difference of sex, or climate.

VII. { ANSERES.
Webbed-footed. Faun. Am. Sept.

29. ANAS, { 44. Marila. 196. 8. Scaup Duck. Br.
Duck, { Zool. Faun. Am. Sept. 17.

Severn River, N° 44 and 45. Fishing Ducks.

Linneus's description, and the figure in the Br. Zoology, folio, plate Q. p. 153, agree perfectly well with the specimens. The female, as Linneus observes, is quite brown, the breast and upper part of the back being of a glossy reddish brown; the speculum of the wing and the belly are white. The eyes of the male have very bright yellow irides; those of the female are of a faint dirty yellow. The female is two ounces heavier than the male, which weighs one pound and an half, is 16½ inches long, and 20 inches broad.

ANAS. 45. Nivalis. Snow Goose. Faun. Am. Sept. p. 16. Lawson's Carolina. Anser niveus Briff.

VI. 288. Klein. Anser nivis. Schwenkfeld, Marfigli. Danub. p. 802. t. 49.

Severn River, N° 40, and a young one, N° 41. white Goose.

These white geese are very numerous at Hudson's Bay, many thousands being annually killed with the gun, for the use of the settlements. They are usually shot whilst on the wing, the Indians being very expert at that exercise, which they learn from their youth; they weigh five or six pounds, are
2½ feet.

2½ feet long, and 3½ broad; their eyes are black, the irides small and red, the legs likewise red; they feed along the sea, and are fine eating; their young are bluish grey, and do not attain a perfect whiteness till they are a year old. They visit Severn river first in the middle of May, on their journey northward, where they breed; return in the beginning of September, with their young, staying at Severn settlement about a fortnight each time. The Indian name is *Way-way*, at Churchill river. Linnaeus has not taken notice of this species.

ANAS. 46. Canadensis. 198. 14. Canada Goose.
Faun. Am. Sept. 16. Edw. 151. Catesby I.
92, &c.
Severn River, N^o 42.

The Canada geese are very plentiful at Hudson's Bay, great quantities of them are salted, but they have a fishy taste. The specimen sent over agrees perfectly with the descriptions and drawings. At Hudson's Bay this species is called the *Small Grey Goose*. Besides this, and the preceding white goose, Mr. Graham, the gentleman who sent the account from Severn settlement, mentions three other species of wild geese to be met with at Hudson's Bay; he calls them,

1. The large Grey Goose.
2. The Blue Goose.
3. The Laughing Goose.

The

The first of these, the large grey goose, he says, is so common in England, that he thought it unnecessary to send specimens of it over. It is however presumed, that though Mr. Graham has shewn himself a careful observer, and an indefatigable collector; yet, not being a naturalist, he could not enter into any minute examination about the species to which each goose belongs, nor from mere recollection know, that his grey goose was actually to be met with in England. A natural historian, by examination, often finds material differences, which would escape a person unacquainted with natural history. The wish, therefore, of seeing the specimens of these species of geese, must occur to every lover of that science. Mr. Graham says, the large grey geese are the only species that breed about Severn river. They frequent the plains and swamps along the coast. Their weight is nine pounds.

The blue goose is as big as the white goose; and the laughing goose is of the size of the Canada or small grey goose. These two last species are very common along Hudson's Bay to the southward, but very rare to the northward of Severn river. The Indians have a peculiar method of killing all these species of geese, and likewise swans. As these birds fly regularly along the marshes, the Indians range themselves in a line across the marsh, from the wood to high water mark, about musket shot from each other,

so as to be sure of intercepting any geese which fly that way. Each person conceals himself, by putting round him some brush wood; they likewise make artificial geese of sticks and mud, placing them at a short distance from themselves, in order to decoy the real geese within shot: thus prepared, they sit down, and keep a good look out; and as soon as the flock approaches, they all lie down, imitating the call or note of geese, which these birds no sooner hear, and perceive the decoys, than they go straight down towards them; then the Indians rise on their knees, and discharge one, two or three guns each, killing two or even three geese at each shot, for they are very expert. Mr. Graham says, he has seen a row of Indians, by calling round a flock of geese, keep them hovering among them, till every one of the geese was killed. Every species of geese has its peculiar note or call, which must greatly increase the difficulty of enticing them.

ANAS. 47. Albeola. 199. 18. The Red Duck.

Faun. Am. Sept. 17. Edw. t. 100. Sarcelle de la Louisiane. Brisson VI. t. 41. f. 1.

Severn River, N^o 37 and 38. Fishing Birds.

The descriptions and figures answer very well with the male, except that the three exterior feathers are not white on the outside, but all dusky.

The female is not described by any one of the ornithologists; and therefore deserves to be noticed,

noticed, to prevent future mistakes. The whole bird is dusky, a few feathers on the forehead are rusty, and some about the ears of a dirty white; the breast is grey, the belly and speculum in the wings white; the bill and legs are black. They visit Severn settlement in June, build their nests in trees, and breed among the woods, and near ponds; the weight of the female is one pound, its length 14 inches, and its breadth 21.

ANAS. 48. Clangula. 201. 23. Golden Eye.
Br. Zool. Faun. Am. Sept. 16.

Severn River, N° 51. .

These birds frequent lakes and ponds, and breed there: they eat fish and slime, and cannot rise off the dry land. The legs and irides are yellow; their weight is 2½ pounds, and their measure 19 inches in length, and two feet in breadth. The specimen sent is the male.

ANAS. 49. Perspicillata. 201. 25. Black Duck.
Faun. Am. Sept. 16. Edw. 155.

Churchill River, N° 14.

This species is exactly described, and well drawn by Edwards. The Indians call it *She-ke-su-partem*. It ought to come into the first division of Linneus's ducks, "rostro basi gibbo," as its bill is really very unequal at the base.

ANAS. 50. Glacialis. 203. 30, and Hyemalis, 202.
 29. Edw. t. 156. Swallow-tail. Br. Zool.
 Faun. Am. Sept. 17.
 Churchill River, N° 12.

At Churchill River the Indians call this species, *Har-bar-vey*; it corresponds with Edwards's description and drawing, plate 156, but differs much from Linneus's inexact description of the *Anas Hyemalis*, to which he, however, quotes Edwards. Upon the whole it is almost without a doubt that the bird represented by Edwards, plate 280, and Br. Zool. folio, plate Q. 7, and quoted by Linneus for his *Anas glacialis*, is the male, and that the bird figured by Edwards t. 156, and quoted by Linneus for the *Anas Hyemalis*, is the female, of one and the same species. Linneus mentions a white body (in his *Anas hyemalis*) which in Edw. Tab. 156, and in the Society's specimen, is all brown and dusky, except the belly, temples, a spot on the back of the head, and the sides of the rump, which are white. Linneus says, that the temples are black; in the specimen now sent over, and in Mr. Edwards's figure, which Linneus quotes, they are white; the breast, back, and wings, are not black as he says, but rather brown and dusky. A further proof, that Linneus's *Anas Glacialis* and *Hyemalis* are the same, is that, the feet in both t. 156 and 280 of Edwards are black, and the bill black, with an orange

ANAS. 31. Crecca. 204. 33. *Varietas*. Teal.
Br. Zool. Faun. Am. Sept. 17.
Severn River, N° 33, 34. Male and female.

This is a variety of the teal, for it wants the two white streaks above and below the eyes; the lower one indeed is faintly expressed in the male, which has also a lunated bar of white over each shoulder; this is not to be found in the European teal. This species is not very plentiful near Severn river; they live in the woods and plains near little ponds of water, and have from five to seven young at a time.

ANAS. 32. *Histrionica*. 204. 35. Harlequin Duck.
Faun. Am. Sept. 16. Edw. t. 99.

This bird had no number fixed to it; it agrees perfectly with Edwards's figure.

ANAS. 53. *Boschas*. 205. 40. Mallard Drake.
Faun. Am. Sept. Br. Zool.

Severn River, N° 39.

It is called Stock Drake at Hudson's Bay, and corresponds in every respect with the European one, upon comparison.

31. *Pelecanus*. 1. 1. *Orientalis*. 1. 1. *Indica*.

York Fort.

This agrees with the peacock, agrees in every particular with Linnæus's oriental pelean (*Pelecanus*).

cannus on-crotalus orientalis), but has a peculiar tuft or fringe of fibres in the middle of the upper mandible, something nearer the apex than the base. This tuft has not been mentioned by any author, and is likewise wanting in Edwards's pelican, t. 92. with which the Society's specimen corresponds in every other circumstance. The *P. Onocrotalus occidentalis*, Linn. or Edw. t. 93 American pelican, is very different from it: the chief differences are the colour, which in our Hudson's Bay bird is white, but in Edwards's is of a greyish brown; and the size, which in the white bird is almost double of the brown one. The quill-feathers are black, and the shafts of the larger ones white. The *Alula*, or ballard wing, is black. The bill and legs are yellow.

22. *COLYMBUS*. } 55. *Glacialis*. 221. 5. Northern
 * Diver, } Diver. Br. Zool. Faun. Am.
 Sept. 16.

Churchill River, N^o 8. called a Loon there.

This bird is well described and drawn in the British Zoology, in folio.

- * * * } 56. *Auritus*, *a.* 222. 8. Edw. 145.
 Grebe. } Eared Grebe. Faun. Am. Sept. 15.
 Severn River, N^o 43.

This is exactly the bird drawn by Edwards, t. 145. The specimen sent over is a female. It differs much from our lesser crested Grebe.

Br.

Br. Zool. octavo I. p. 396, and Br. Zool. illustr. plate 77. fig. 2. and Ed. 96. fig. 2. However, in both these works, it is looked on only as a variety, or different in sex. Mr. Graham has the same opinion. It lives on fish, frequenting the lakes near the sea coast. It lays its eggs in water, and cannot rise off dry land. It is seen about the beginning of June, but migrates southward in autumn. It is called *Sekeep*, by the natives. Its eyes are small, the irides red; it weighs one pound, and measures one foot in length, and one third more in breadth.

23. LARUS. } 57. Parasiticus. 226. 10. Arctic Gull.
Gull. } Br. Zool. Faun. Am. Sept. 16. Edw.

148. 149.

Churchill River, N^o 15.

This species is called a *Man of War*, at Hudson's Bay. It seems to be a female, by the dirty white colour of its plumage below; it agrees very well with Edwards's drawing, and that in the Br. Zool. illustr.

24. STERNA. } 58. Hirundo (*Variety*), 227. 2.
Tern. } The greater Tern. Br. Zool. Faun.
Am. Sept.

(The number belonging to this bird is lost, perhaps it is N^o 17, from Churchill River, called

" A fort

"A sort of Gull, called Egg-breakers, by
"the natives.")

The feet are black; the tail is shorter and
much less forked than that described and
drawn in the Br. Zool. The outermost tail-
feather likewise wants the black, which that
in the British Zoology has. In other re-
spects it is the same.

DESCRIPTIONES Avium Rariorum e Sinu Hudsonis.

I. FALCO SACER.

FALCO, cœrâ pedibusque coeruleis, corpore, remi-
gibus rectricibusque fuscis, fasciis pallidis; capite,
pectore & abdomine albis, maculis longitudinalli-
bus fuscis.

Habitat ad sinum Hudsonis et in reliqua America
Septentrionali; victitat Lagopodibus & Tetraonum
speciebus.

DESCR. *Magnitudo Corvi.*

Rostrum, cœrâ, pedes coerulei; rostrum
breve, curvum, coeruleo-atrum; mandi-
bula utraque, basi pallide coerulea, apice
nigrescente, utraque emarginata.

Caput tectum pennis albidis, maculis longi-
tudinalibus, fuscis.

Oculi magni; irides flavæ.

Gula alba; fusco-maculata.

Dorsum et rectrices alarum, plumis fuscis,
ferrugineo-pallide marginatis, maculatis-
que, maculis rachin non attingentibus.

Alæ, venter, crurum, rectrices alarum
inferiores, & femora alba, maculis longi-
tudinalibus nigro-fuscis.

Remiges fuscæ albis, apicibus duo; primo-
res apicibus marginibus albis, maculis fer-

rugineo-pallidis, intra in joribus, transversis, extra mincribus, rotundatis.

Refrices duodecim, supra fuscæ, latellis circiter duodecim & apice albidis; intra cinereæ, latell. albidis.

2. *SIRIX NEBULOSA.*

SIRIX capite laevi, corpore fusco, albido undulatum striato, remige sexto longiore, apice nigricante.

Habitat circa Sinum Hudsonis, viciitat Lepontibus, Lagopodibus, Muribusque.

DESCR. *Rostrum* fusco-flavum, mandibula superiore superius magis flava.

Oculi magni, iridibus flavis.

Caput facie cinerea, e pennis fusco et pallide cinereo alternatim striatis. Pone hæc pennas collum versus est ordo plumularum fuscarum ad utramque genam, semicirculum nigrum efficiens.

Occiput, cervix, et collum fusca, pennis, marginibus albo-maculatis.

Pectus albidum, maculis longitudinalibus transversisque fuscis.

Abdomen album, superius uti pectus maculis longitudinalibus, sed inferius striis transversis notatum.

Dorsum totum et tectrices alæ, caudæque confertim ex fusco & albido undulato-striata.

Alæ fusca; remiges primores fusci, griseo transversim fasciati, fasciis latis nebulosis.

Remex sextus, reliquis longior, apice magis

magis nigricans; primus vero reliquis primoribus brevior. Remiges reliqui pallidiores, obscurius fasciati.

Cauda rotundata, rectricibus duodecim: duæ intermediæ paullo longiores, totæ cinerascens albedo fuscoque undulatum striatæ, lineis duplicatis fuscis transversis pluribus. Rectrices reliquæ fuscæ albedo substriatæ.

Pedes tecti pennis albidis fusco-striatis.

Magnitudo fere Strigis Nyctææ, Linn.

Longitudo unciarum 16 pedis Anglicani.

Latitudo pedum quatuor.

Pondus librarum trium.

3. TETRAO PHASIANELLUS.

Linn. Ed. X. p. 160. n. 5.

TETRAO pedibus hirsutis, cauda cuneiformi, remigibus nigris, exterius albo-maculatis.

Habitat ad Sinum Hudsonis.

DESCR. *Magnitudo* fere Tetraonis Tetricis. Linn.

Rostrum nigrum.

Oculorum irides avellanæ.

Caput, collum & dorsum testacea, nigro transversim fasciata: macula albida inter rostrum et oculos: latera colli notata maculis rotundatis albidis.

Dorsum testaceum, plumis omnibus late nigro-fasciatis.

Uropygium magis albedo-cinereum, nigredine fimbriata secundum rachin plumarum.

Pectus & Venter albida, maculis cordatis fusco-testaceis in ventre saturatioribus.

Alarum rectrices dilute testaceo, nigro, alboque transversim fasciatæ, maculis pluribus rotundis albis. *Remiges* primores nigri, latere exteriori albo-maculati; secundarii fusci, apice & ad marginem anteriorem albo subfasciati: postremi vero testaceo fasciati, apice tantum albi.

Rectrices breves, exteriores pallide fuscae, apice albæ, duæ intermediæ reliquis longiores, testaceo-maculatæ.

Pedes plumis albo-griseis vestiti digitis pectinatis.

Longitudo unciarum 16 pedis Anglicani.

Latitudo pedum duorum.

4. EMBERIZA LEUCOPHREYS *.

EMBERIZA remigibus rectricibusque fuscis, capite nigro, fascia verticis, superciliisque niveis.

Habitat in America Boreali ad Sinum Hudsonis.

DESCR. *Magnitudo* circiter *fringillæ* *cælibis*.

Rostrum rubrum, s. carnei coloris: Nares subrotundæ.

Caput fascia verticali lata candida, paululum ante rostrum desinente; fascia atra

* *Emberiza* *leucophrys*. *Caput* supercilium.

lata ad utrumque latus fasciæ albæ. Supercilia alba, desinentia in lineas, fasciam albam verticalem adtingentes; arcus dein atri, ex angulis oculorum, fere in occipite confluentes.

Collum cinerascens, in pectore dilutius.

Dorsum ferrugineo-fuscum, marginibus plumularum cinereis.

Alæ fuscae; remigum primorum margines exteriores tenuissimi pallidi, interiores cinerascens: secundarii & pennæ tectrices fuscae, marginibus latiusculis, versus apicem albis, efficientibus fasciam albam; super quam fascia altera alba ex maculis albis in apice tectricum minorum, s. plumarum scapularium. Alulæ albæ. Remiges subtus cinerei, marginibus albis.

Pectus cinereum, abdomen dilutius, fere album.

Crissum & plumulæ femora tegentes lutescentia.

Uropygium cinereo-fuscum.

Cauda æqualis; rectrices duodecim fuscae, marginibus paullo pallidioribus, subtus cinereæ.

Pedes carnei coloris, digito intermedio & ungue postico reliquis longioribus.

Longitudo unciarum 7 pedis Anglicani.

Latitudo inter alas extensas 9 unciarum pedis Anglicani.

Cauda partem tertiam longitudinis totius aviculæ efficit.

Alæ complicatæ paululum ultra caudæ-
exortum protenduntur.

Pondus drachmarum sex.

5. FRINGILLA HUDSONIAS.

FRINGILLA fusco-cinereascens, rostro albedo, pec-
tore inferiore, abdomine, rectricibusque quatuor
extremis albis.

Habitat in America Boreali.

DESCR. *Magnitudo* circiter fringillæ carduelis.

Rostrum albidum, rubedine aliqua imbu-
tum.

Oculi parvi, cœrulei.

Corpus totum cinereo-nigricans, s. potius
fuliginosum.

Pectus inferius & *abdomen* alba.

Remiges fusci, cinereo-marginati : alæ
complicatæ mediam fere caudam ad-
tingunt.

Rectrices fuscæ, extimæ utrinque duæ totæ
albæ, tertia fusca, macula oblonga alba,
ad latus interius, prope rachin, apicem
attingens ; reliquæ totæ fuscæ.

Pondus semuncie.

Longitudo unciarum 6½ pedis Anglicani.

Latitudo unciarum novem.

6. MUSCICAPA STRIATA.

MUSCICAPA cinereo-virens, dorso nigro striato, sub-
tus flavescenti-alba, gula lateribusque pectoris
fusco maculatis.

Habitat

Habitat ad Sinum Hudsonis.

Quum mas à scemina multum differat, utique congruum est, utrumque sexum separatim describere.

DESCR. Mas.

Rostrum trigonum, manlibu sup riore paululum longiore, ante apicem leviter emarginata, nigra; inferiore basi flavescente.

Nares subrotundæ.

Vibrissæ nigrae.

Caput supra totum atrum ad oculos usque.

Genæ à rostro in occiput totæ albæ; occiput albo & nigro variegatum.

Gula flavescenti-alba maculis fuscis.

Pectus albidum, lateribus, five versus occiput maculis nigris variegatum.

Dorsum cinereo-virens, striis five maculis longitudinalibus nigris latioribus, è plumulis nigris, margine virentibus.

Abdomen album.

Uropygium cinereum, nigro-maculatum.

Alæ fuscae; remiges primores pallido marginati, secundarii apice tenuissimo albo; duæ ultimæ margine exteriori albo; rectrices fuscae, majores flavescenti albo, minores candido in apice maculatæ, unde fasciæ albæ binæ in alis.

Cauda fusca; rectrix utrinque prima s. extrema, latere interiore macula magna alba, marginem interiorem attingente; proxima s. secunda macula oblonga minore alba, etiam marginem interiorem attingente;

attingente; utrinque tertia, latere interiore versus apicem albo-marginata.

Pedes lutei; ungues breves, pallide fusci.

Magnitudo circiter *Pari atricapilli*; Linn.

Longitudo 5 unciarum.

Latitudo 7 unciarum pedis Anglicani.

Fœmina.

Rostrum, alæ, cauda, abdomen, uropygium, pedes & mensuræ ut in mare.

Caput flavo-virens, striis brevibus tenuibusque longitudinalibus nigris; linea flavissima à basi rostri incipiens super oculos ducta; palpebræ flavæ.

Gula, genæ & pectus albido-flava; maculæ spariæ oblongiusculæ fuscæ, ab utroque oris angulo ulque in pectoris latera.

Dorsum, ut in mare, sed viridius, & striæ nigrae minores.

7. PARUS HUDSONICUS.

PARUS capite fusco-rubescente, dorso cinereo, jugulo atro, fascia suboculari, pectoreque albis, hypochondriis rufis.

Habitat ad Sinum Hudsonis.

DESCR. *Rostrum* subulatum, integerrimum, atrum, basi è regione narium tectum fasciulis setarum ferruginearum, lineas 4 (unciarum pedis Anglicani) longum.

Caput fusco-ferrugineum, fascia sub oculis alba; gula atra, nigredine extensa sub hac fascia alba.

Dorsum

Dorsum cinereo-virens, è plumis longioribus, fuscis, apice tantum cinereo-virentibus, s. olivaceis.

Pectus & Abdomen alba, sed plumæ omnes basi nigræ, apice tantum albæ.

Latera abdominis & lumbi ferruginei.

Alæ fuscæ, remigum margine omni cinereo.

Cauda fusca, rotundata, rectricibus 12, margine cinereis.

Uropygium tectum plumulis aliquot nigris, apice albido-rufis.

Pedes nigri; digitus posticus cum ungue anticorum digitorum medio, duplo longior.

Longitudo unciarum 5½. pedis Anglicani.

Latitudo unciarum 7.

Cauda uncias 2½. longa.

8. SCOLOPAX BOREALIS.

SCOLOPAX rostro arcuato, pedibusque nigris, corpore fusco, griseo-maculato, subtus ochroleuco.

Habitat in Sinus Hudsonis inundatis, & pratis humidis, victitans vermibus & insectis: mense Aprili vel initio Maii primum visa est, circa Castellum Albany, inde in terras magis arcticas migrat, ibique nidificat; redit ad idem castellum mense Augusto; regiones Australiores petit circa finem Septembris.

Affinis scolopace arcuata Linn. sed differt corpore triplo minore, rostro ratione corporis brevior.

breviore, colore in dorso saturate fusco, in abdomine ochroleuco.

DESCR. *Caput* pallidum, lineolis confertis longitudinalibus fuscis : sinciput saturate fuscum, pallido maculatum.

Rostrum nigricans, arcuatum, longitudine duarum unciarum pedis Anglicani, mandibula inferiore basi rufa.

Collum, pectus, abdomen & crissum ochroleuca ; pectore colloque lineolis longitudinalibus fuscis confertioribus, abdomine & crisso fere nullis, vel tenuibus notatis.

Femora semi-tectis plumulis ochroleucis, fusco maculatis.

Latera abdominis sub alis præsertim, rufa, pennis transversim fusco fasciatis.

Dorsum totum saturate fuscum, pennis margine albido griseis.

Alæ fuscae ; remiges primores immaculati, primores rachi tota alba ; reliqui, s. secundarii pallide griseo-marginati. Tectrices late griseo-marginatæ. Tectrices inferiores alæ, ferrugineæ fusco transversim fasciatæ. Alæ complicatæ fere mediam caudam attingunt.

Uropygium fuscum, marginibus maculisque pennarum albidis.

Cauda brevis, fusca, rectricibus albido transversim fasciatis

Pedes nigri, s. cœrulescentes.

Longitudo unciarum 13½.

Latitudo circiter unciarum 2½.

9. ANAS NIVALIS.

ANAS, rostro cylindrico, corpore albo, remigibus
primoribus nigris.

Habitat in America Boreali, per Sinum Hudsonis
migrans.

DESCR. *Corpus* totum album, magnitudine anseris
domestici nostratis.

Rostrum luteum, mandibulis subserratis.

Oculi iride rubra.

Remiges decem primores nigri, scapis al-
bis: teſtrices infimæ cineræ, scapis ni-
gris; pennæ duæ alulæ, itidem ci-
neræ, scapis nigris.

Pedes rubri.

Longitudo pedum duorum 8c unciarum
oſto.

Latitudo pedum $3\frac{1}{2}$.

Pondus librarum 5 vel 6.

XXX. *Geometrical Solutions of three celebrated Astronomical Problems, by the late Dr. Henry Pemberton, F. R. S. Communicated by Matthew Raper, Esq; F. R. S.*

LEMMA.

Read June 4,
1772.

TO form a triangle with two given sides, that the rectangle under the sine of the angle contained by the two given sides, and the tangent of the angle opposite to the lesser of the given sides, shall be the greatest that can be.

Let [TAB. XII. Fig. 1.] the two given sides be equal to AB and AC: round the center A, with the interval AC, describe the circle CDE, and produce BA to E; take BF a mean proportional between BE and BC, and erect the perpendicular FG, and complete the triangle AGB.

Here the sine of BAG is to the radius, as FG to AG; and the tangent of ABG to the radius, as FG to FB: therefore, the rectangle under the sine of BAG and the tangent of ABG is to the square of the

the

Fig. 1.

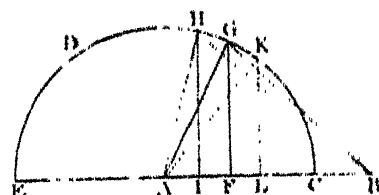


Fig. 2.

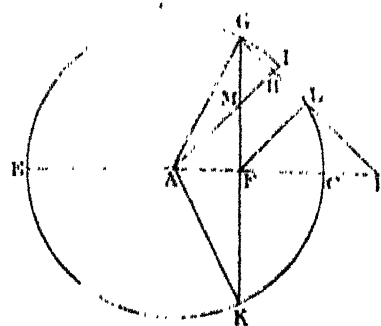


Fig. 3.

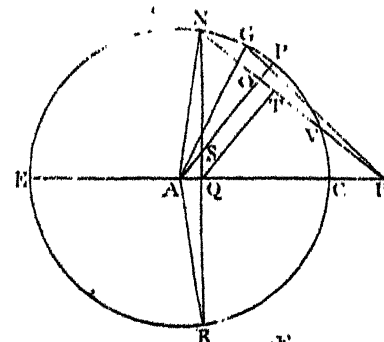


Fig. 4.

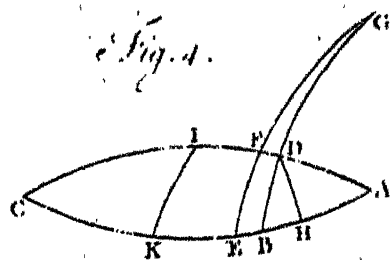


Fig. 5.

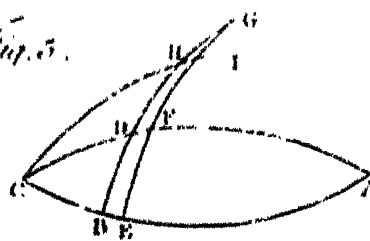


Fig. 6.

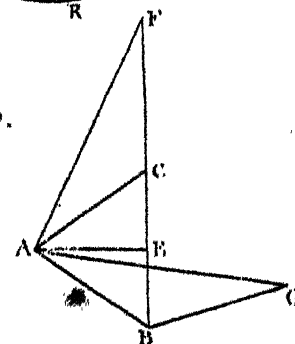


Fig. 7.

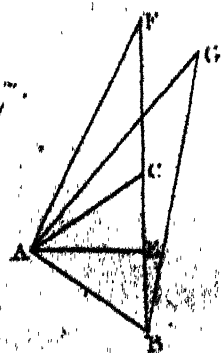


Fig. 8.

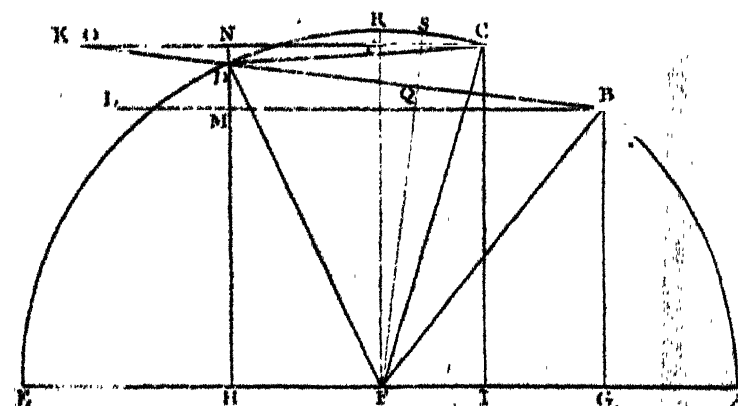
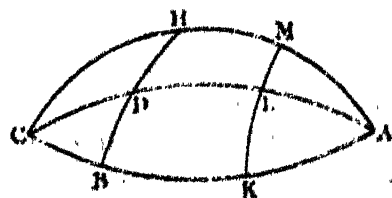


Fig. 9.

the radius, as the square of FG , or the rectangle EFC , to the rectangle under AG (or AC) and FB . But, EB being to BF as BF to BC , by conversion, EB is to EF as BF to FC , and also, by taking the difference of the antecedents and of the consequents, EF is to twice AF as BF to FC ; and twice AFB is equal to EFC .

Now, let the triangle BAH be formed, where the angle BAH is greater than BAG . Here, the perpendicular HI being drawn, the rectangle under the sine of BAH and the tangent of ABH will be to the square of the radius, as the rectangle EIC to the rectangle under AC , IB . But IF is to FB as $2AFI$ to $2AFB$, or EFC ; and $2AFI$ is greater than $AF^2 - AI^2$; also $AF^2 - AI^2$ together with EFC , is equal to EIC ; therefore, by composition, the ratio of IB to BF is greater than that of EIC to EFC ; and the ratio of $AC \times IB$ to $AC \times FB$ greater than that of EIC to EFC : also, by permutation, the ratio of $AC \times IB$ to EIC greater than the ratio of $AC \times FB$ to EFC . But the first of these ratios is the same with that of the square of the radius to the rectangle under the sine of BAH and the tangent of ABH ; and the latter is the same with that of the square of the radius to the rectangle under the sine of BAG and the tangent of ABG ; therefore, the latter of these two rectangles is greater than the other.

Again, let the triangle BAK be formed, with the angle BAK less than BAG , and the perpendicular KL be drawn. Then the rectangle under the sine of BAK and the tangent of ABK is to the square of the radius, as the square of KL to the rectangle under

AC, BL. Here, FL being to FB as 2 AFL to 2 AFB or EFC , and 2 AFL less than $\text{AL}^2 - \text{AF}^2$, by conversion, the ratio of LB to FB will be greater than the ratio of ELC to EFC; therefore, as before, the rectangle under the sine of BAG and the tangent of ABC is greater than that under the sine of BAK and the tangent of ABK.

COROLLARY I.

BF is equal to the tangent of the circle from the point B; therefore, BF is the tangent, and AB the secant, to the radius AC, of the angle, whose cosine is to the radius as AC to AB. Therefore, AF is the tangent, to the same radius, of half the complement of that angle; and AF is also the cosine of the angle BAG to this radius.

COROL. 2.

The sine of the angle composed of the complement of AGB, and twice the complement of ABG, is equal to three times the sine of the complement of AGB. Let fall the perpendicular AH (Fig. 2.), cutting the circle in I; continue GF to K, and draw AK. Then $\text{BF} = \text{EB} = \text{GBL}$. Therefore, $\text{GB} : \text{BF} :: \text{BF} : \text{BL}$, and the triangles GBF, FBL are similar. Consequently FL is perpendicular to GB, and parallel to AH; whence GH being equal to HL, GM is equal to MF, and MK equal to three times GM.

Now, the arc $\text{IK} = 2 \text{ IC} + \text{GI}$; and the angle $\text{IAK} = 2 \text{ IAC} + \text{GAI}$; also GM is to MK as
the

the sine of the arc GI to the sine of the arc IK, that is, as the sine of the angle GAI to the sine of the angle IAK. Therefore, the sine of the angle IAK ($= 2IAC + GAI$) is equal to three times the sine of the angle GAI; but GAI is the complement of AGB, and IAC the complement of ABG.

COROL. 3.

If (Fig. 3.) any line BN be drawn to divide the angle ABG, and AN be joined, also AO be drawn perpendicular to BN, and continued to the circle in P, the sine of the angle composed of NAP and $2PAC$ will be less than three times the sine of the angle NAP. Draw NQR perpendicular to AB, cutting AP in S; join AR, and draw QT perpendicular to BN, and parallel to AO; then $BQ = NB$. But BQ is greater than the rectangle EBC, that is, greater than the rectangle NBV, under the two segments of the line BN drawn from B, to cut the circle in N and V: therefore, TB is greater than VB, and NO greater than OT. Consequently NS is greater than SQ. Hence RS is less than three times NS; and therefore, the sine of the angle PAR ($= NAP + 2PAC$) is less than three times the sine of NAP.

PROBLEM I.

To find in the ecliptic the point of longest ascension.

ANALYSIS.

Let (Fig. 4.) ABC be the equator, ADC the ecliptic, BD the situation of the horizon, when D is the point of longest ascension. Let $EF'G$ be another situation of the horizon. Then the ratio of the sine of EB to the sine of FD is compounded of the ratio of the sine of BG to the sine of GD , and of the ratio of the sine of AE to the sine of AF ; but the angles B and E being equal, the arcs EG , GB together make a semicircle; and, by the approach of EG towards GB , the ultimate magnitude of BG will be a quadrant, and the ultimate ratio of EB to FD will be compounded of the ratio of the radius to the sine of DG (that is, the cosine of BD) and of the ratio of the sine of AB to the sine of AD . Draw the arc DH perpendicular to AB . Then, in the triangle BDH , the radius is to the cosine of BD , as the tangent of the angle BDH to the cotangent of HBD . Also, in the triangle BDA , the sine of AB is to the sine of AD as the sine of the angle BDA (or BDC) to the sine of ABD ; therefore, the ultimate ratio of BE to DF is compounded of the ratio of the tangent of BDH to the cotangent of ABD , and of the ratio of the sine of BDC to the sine of ABD ; which two ratios compound that of the rectangle under the tangent of BDH and the sine of BDC to the rectangle under the cotangent and the sine of the given angle ABD .

But, when D is the point of longest ascension, the ratio of BE to DF is the greatest that can be; therefore, then the ratio of the rectangle under the tangent of BDH and the sine of BDC to the given rectangle under the cotangent and sine of the given angle ABD must be the greatest that can be; and consequently, the rectangle under the tangent of BDH , and the sine of BDC , must be the greatest that can be.

In the triangle BDA , the sine of BDH is to the sine of HDA , as the cosine of ABD to the cosine of BAD . Now, in the preceding lemma, let the angle BAG of the triangle AGB be equal to the spherical angle BDC : then will the sum of the angles ABG , AGB be equal to the spherical angle BDA . And, if AG in the triangle AGB , be to AB as the cosine of the spherical angle DBA to the cosine of DAB , that is, as the sine of BDH to the sine of HDA , the angle ABG , in the triangle, will be equal to the spherical angle BDH ; and the angle AGB , in the triangle, equal to the spherical angle HDA . Therefore, by the first corollary of the lemma, that the rectangle under the tangent of the spherical angle BDH and the sine of BDC be the greatest that can be; the cosine of BDC must be equal to the tangent of half the complement of the angle, whose cosine is to the radius, as AG to AB , in the triangle, or as the cosine of the spherical angle ABD to the cosine of the spherical angle BAD .

If IK be the situation of the horizon, when the solstitial point is ascending, in the quadrantal triangle AIK , the cosine of KIC is to the radius as the cosine of IKA ($= DBA$) to the cosine of IAK . Therefore,

fore, the cosine of BDC , when D is the point of longest ascension, is equal to the tangent of half the complement of the angle, which the ecliptic makes with the horizon, when the solstitial point is ascending.

But, the sine of the angle composed of DAB , and twice ABD , must be less than three times the sine of the angle BAD . In the spherical triangle ABD , the angles BAD , ABD together exceed the external angle BDC . Therefore, in the third corollary of the lemma, let the angle BAN be equal to the sum of the spherical angles BAD , ABD : but here, AN is to AB as the cosine of the spherical angle ABD to the cosine of BAD ; and AN is also to AB as the sine of ABN to the sine of ANB , that is, as the cosine of BAP to the cosine of NAP ; consequently, since the angle BAN is equal to the sum of the spherical angles BAD , ABD , the angle NAP is equal to the spherical angle BAD , and the angle BAP equal to the spherical angle ABD ; but the sine of the angle composed of NAP and twice PAB is less than three times the sine of NAP ; therefore, the sine of the angle composed of the spherical angle BAD and 2 ABD will be less than three times the sine of the angle BAD ; otherwise no such triangle DBA , as is here required, can take place, but the point A will be the point of longest ascension.

If the sine of the angle A be greater than one third of the radius, the point A can never be the point of longest ascension; but when the sine of this angle is less, the angle compounded of BAD and twice ABD , may be greater or less than a quadrant;
and

and therefore, the magnitude of the angle ABD, that A be the point of longest ascension, is confined within two limits, of which the double of one added to the angle A, as much exceeds a quadrant, as the double of the other added to that angle falls short of it; therefore, double the sum of those two angles, together with twice A, makes a semicircle; and the single sum of those two angles added to A makes a quadrant.

PROBLEM II.

To find when the arc of the ecliptic differs most from its oblique ascension.

ANALYSIS.

If (Fig. 5.) BD be the situation of the horizon, when CD differs most from CB, as before, the ultimate ratio of BE to DF will be compounded of the ratio of the radius to the sine of DG (or the cosine of DB) and of the ratio of the sine of CB to the sine of CD: but, when CD differs most from CB, BE and DF are ultimately equal; therefore, then the cosine of BD is to the radius as the sine of CB to the sine of CD.

Draw the arc CHI of a great circle, that DH be equal to DB; then, BH being double BD, half the sine of BH is to the sine of BD or DH, as the cosine of BD to the radius; therefore, half the sine of BH is to the sine of DH as the sine of CB to the sine of CD; but the sine of the angle BCH is to the sine of BH as the sine of the angle CHB to the

sine of CB ; whence, by equality, half the sine of BCH is to the sine of DH as the sine of CHB to the sine of CD : but as the sine of CHB to the sine of CD , so, in the triangle CHD , is the sine of DCH to the sine of HD : consequently, the sine of DCH is equal to half the sine of BCH . Hence, the difference of the angles BCH , DCH being given, those angles are given, and the arc CH is given by position.

Moreover, in the triangle BCH , the base BH being bisected by the arc CD , the sine of the angle CHD is to the sine of the given angle CBD , as the sine of the given angle HCD to the sine of the given angle BCD ; therefore, the angle CHB is given; in so much, that in the triangle CBH all the angles are given.

The sum of the sines of the angles BCH , DCH is to the difference of their sines, as the tangent of half the sum of those angles to the tangent of half their difference; therefore, the tangent of half the sum of BCH , DCH is three times the tangent of half BCD .

In (Fig. 6.) the isosceles triangle ABC , let the angle BAC be equal to the spherical angle BCD , and let AE be perpendicular to BC ; also, CF being taken equal to CB , join AF : then EF is equal to three times EB ; and as EF to EB , so is the tangent of the angle EAF to the tangent of EAB ; but EAB is equal to half the spherical angle BCD : therefore, the angle EAF is equal to half the sum of the spherical angles BCD , BCH ; and consequently, the angle CAF equal to the spherical angle DCH . Here, AF is to CF as the sine of the angle ACF

to the sine of CAF ; and CB is to AB as the sine of the angle BAC to the sine of ACB : therefore, CF being equal to CB , and the sine of ACF to the sine of ACB , by equality, AF is to AB as the sine of the angle BAC to the sine of CAF , that is, as the sine of the spherical angle BCD to the sine of the spherical angle DCH .

Let (Fig. 7.) the triangle AGB have the angle ABG equal to the spherical angle CBD , and the side AG equal to AF . Then, AG is to AB as the sine of the spherical angle BCD to the sine of the spherical angle DCH , that is, as the sine of the spherical angle CBH to the sine of the spherical angle CHB : but AG is to AB also as the sine of the angle ABG to the sine of AGB ; therefore, the angle ABG being equal to the spherical angle CBH , the angle AGB is equal to the spherical angle CHB : and moreover, when the angle ABG is greater than ABF , that is, when the spherical angle CBH is greater than the complement of half BCD , the three angles ABG , AGB and BAC together exceed two right.

Hence, (Fig. 8.) towards the equinoctial point C , where the angle CBD is obtuse, a situation of the horizon, as BD , may always be found, wherein CD more exceeds CB than in any other situation: and when the acute angle DBA is greater than the complement of half BCD , another situation of the horizon, as KLM , may be found, toward the other equinoctial point A , wherein the arc of the ecliptic CK will be less than the arc of the equator, and their difference be greater than in any other situation. But, if the angle DBA be not greater than the complement

plement of half BCD , the arc of the ecliptic, between C and the horizon, will never be less than the arc of the equator, between the same point C and the horizon.

In the two situations of the horizon, the angles CHB and KMA are equal.

SCHOLIUM 1.

To find the point in the ecliptic, where the arc of the ecliptic most exceeds the right ascension, is a known problem: that point is, where the cosine of the declination is a mean proportional between the radius and the cosine of the greatest declination.

In the preceding figure, supposing the angle CBD to be right, then, because when CD most exceeds CB , the cosine of BD is to the radius as the sine of CB to the sine of CD , and, in the triangle CBD , the sine of CB is to the sine of CD as the sine of the angle CDB to the radius, also the sine of CDB is to the radius as the cosine of BCD to the cosine of BD ; therefore, the cosine of BD is to the radius as the cosine of the angle BCD to the cosine of the same BD , and the cosine of BD is a mean proportional between the radius and the cosine of BCD .

SCHOLIUM 2.

In any given declination of the Sun, to find when the azimuth most exceeds the angle which measures the time from noon, is a problem analogous to the preceding.

Dr.

PROBLEM III.

*The tropic found, by Dr. Halley's method *, without any consideration of the parabola.*

The observations are supposed to give the proportions between the differences of the sines of three declinations of the Sun near the tropic ; but the sine of the Sun's place is in a given proportion to the sine of the declination ; therefore, the same observations give equally the proportion between the differences of the sines of the Sun's place, in each observation.

Now (Fig. 9.), let ACE be the ecliptic, AE its diameter between Υ and φ , and its center F ; let B, C, D be three places of the Sun ; BG, CI, DH the sines of those places respectively. Draw CK, BL parallel to AE, which may meet HD, in N and M. Then, by the observations, the ratio of DM to DN is given. Therefore, if BD be drawn to meet KL in O, the ratio of BD to OD is given ; and the ratio of BD to DC is also given, they being the chords of the given angles BFD, CFD : hence the ratio of CD to DO, in the triangle CDO, is given ; and consequently, the angle COD will be given : which angle is the distance of the tropic from the middle point of the ecliptic between B and D : for, FPR being perpendicular to OC, and FQS perpendicular to DB, the angle QFP is equal to QOP, the points O, P, Q, F, being in a circle.

* Vide Philosophical Transactions, N° 215.

THE CALCULATION.

$$\left. \begin{array}{l} DN : DM \\ \text{f. } \frac{1}{2} BFD : \text{f. } \frac{1}{2} CFD \end{array} \right\} :: \text{rad.} : \text{t. } \angle \chi$$

$$\text{rad.} : \text{t. } \angle \chi \approx 45^\circ :: \text{t. } \frac{1}{2} BFC : \text{t. } \frac{COD}{2} \approx \frac{DCO}{2}$$

If $\chi > 45^\circ$, $\angle COD > DCO$

And

if $\chi < 45^\circ$, $\angle COD < DCO$.

If the intervals between the observations are so small, that the lines differ not much from the arches, the arches BC, CD may be counted in time, and the calculation may be abbreviated thus :

$$DM : DN :: \text{arc. } BD : Z \text{ (for DO)}$$

$$DC + Z : 2 DC :: \frac{1}{2} BC : SR.$$

Or,

$$DM \times DC + DN \times BD : DM \times DC :: \frac{1}{2} BC : SR.$$

Received May 18, 1772.

XXXI. *On the Digestion of the Stomach after Death, by John Hunter, F. R. S. and Surgeon to St. George's Hospital.*

Read June
18, 1772.

AN accurate knowledge of the appearances in animal bodies that die of a violent death, that is, in perfect health, or in a sound state, ought to be considered as a necessary foundation for judging of the state of the body in those that are diseased.

But as an animal body undergoes changes after death, or when dead, it has never been sufficiently considered what those changes are; and till this be done, it is impossible we should judge accurately of the appearances in dead bodies. The diseases which the living body undergoes (mortification excepted) are always connected with the living principle, and are not in the least similar to what may be called diseases or changes in the dead body: without this knowledge, our judgment of the appearances in dead bodies must often be very imperfect, or very erroneous; we may see appearances which are natural, and may suppose them to have arisen from disease; we may see diseased parts, and suppose them in a natural state; and we may suppose a circumstance to have existed before

fore death, which was really a consequence of it; or we may imagine it to be a natural change after death, when it was truly a disease of the living body. It is easy to see therefore, how a man in this state of ignorance must blunder, when he comes to connect the appearances in a dead body with the symptoms that were observed in life; and indeed all the usefulness of opening dead bodies depends upon the judgement and sagacity with which this sort of comparison is made.

There is a case of a mixed nature, which cannot be reckoned a process of the living body, nor of the dead; it participates of both, inasmuch as its cause arises from the living, yet cannot take effect till after death.

This shall be the object of the present paper; and, to render the subject more intelligible, it will be necessary to give some general ideas concerning the cause and effects.

An animal substance, when joined with the living principle, cannot undergo any change in its properties but as an animal; this principle always acting and preserving the substance, which it inhabits, from dissolution, and from being changed according to the natural changes, which other substances, applied to it, undergo.

There are a great many powers in nature, which the living principle does not enable the animal matter, with which it is combined, to resist, viz. the mechanical and most of the stronger chemical solvents. It renders it however capable of resisting the powers of fermentation, digestion, and perhaps several others, which are well known to
act

act on this same matter, when deprived of the living principle, and entirely to decompose it. The number of powers, which thus act differently on the living and dead animal substance, is not ascertained: we shall take notice of two, which can only affect this substance when deprived of the living principle; which are, putrefaction and digestion. Putrefaction is an effect which arises spontaneously; digestion is an effect of another principle acting upon it, and shall here be considered a little more particularly.

Animals, or parts of animals, possessed of the living principle, when taken into the stomach, are not the least affected by the powers of that viscous, so long as the animal principle remains; thence it is that we find animals of various kinds living in the stomach, or even hatched and bred there: but the moment that any of those lose the living principle, they become subject to the digestive powers of the stomach. If it were possible for a man's hand, for example, to be introduced into the stomach of a living animal, and kept there for some considerable time, it would be found, that the dissolvent powers of the stomach could have no effect upon it; but if the same hand were separated from the body, and introduced into the same stomach, we should then find that the stomach would immediately act upon it.

Indeed, if this were not the case, we should find that the stomach itself ought to have been made of indigestible materials; for, if the living principle was not capable of preserving animal

substances from undergoing that process, the stomach itself would be digested.

But we find on the contrary, that the stomach, which at one instant, that is, while possessed of the living principle, was capable of resisting the digestive powers which it contained, the next moment, *viz.* when deprived of the living principle, is itself capable of being digested, either by the digestive powers of other stomachs, or by the remains of that power which it had of digesting other things.

From these observations, we are led to account for an appearance which we find often in the stomachs of dead bodies; and at the same time they throw a considerable light upon the nature of digestion. The appearance which has been hinted at, is a dissolution of the stomach at its great extremity; in consequence of which, there is frequently a considerable aperture made in that *viscus*. The edges of this opening appear to be half dissolved, very much like that kind of dissolution which fleshy parts undergo when half digested in a living stomach, or when dissolved by a caustic alkali, *viz.* pulpy, tender, and ragged.

In these cases the contents of the stomach are generally found loose in the cavity of the *abdomen*, about the spleen and diaphragm. In many subjects this digestive power extends much further than through the stomach. I have often found, that after it had dissolved the stomach at the usual place, the contents of the stomach had come into contact with the spleen and diaphragm,

had partly dissolved the adjacent side of the spleen, and had dissolved the diaphragm quite through; so that the contents of the stomach were found in the cavity of the *thorax*, and had even affected the lungs in a small degree.

There are very few dead bodies, in which the stomach is not, at its great end, in some degree digested; and one who is acquainted with dissections, can easily trace the gradations from the smallest to the greatest.

To be sensible of this effect, nothing more is necessary, than to compare the inner surface of the great end of the stomach, with any other part of the inner surface; what is found, will appear soft, spongy, and granulated, and without distinct blood vessels, opaque and thick; while the other will appear smooth, thin, and more transparent; and the vessels will be seen ramifying in its substance, and upon squeezing the blood which they contain from the larger branches to the smaller, it will be found to pass out at the digested ends of the vessels, and appear like drops on the inner surface.

These appearances I had often seen, and I do suppose that they had been seen by others; but I was at a loss to account for them; at first, I supposed them to have been produced during life, and was therefore disposed to look upon them as the cause of death; but I never found that they had any connection with the symptoms: and I was still more at a loss to account for these appearances when I found that they were most frequent in those who died of violent deaths, which made

me suspect that the true cause was not even imagined*.

At this time I was making many experiments upon digestion, on different animals, all of which were killed, at different times, after being fed with different kinds of food; some of them were not opened immediately after death, and in some of them I found the appearances above described in the stomach. For, pursuing the enquiry about digestion, I got the stomachs of a vast variety of fish, which all die of violent deaths, and all may be said to die in perfect health, and with their stomach commonly full; in these animals we see the progress of digestion most distinctly; for as they swallow their food whole, that is, without mastication, and swallow fish that are much larger than

* The first time that I had occasion to observe this appearance in such as died of violence and suddenly, and in whom therefore I could not easily suppose it to be the effect of effluvia in the living body, was in a man who had his skull fractured and was killed outright by one blow of a poker. Just before this accident, he had been in perfect health, and had taken a hearty supper of cold meat, cheese, bread, and ale. Upon opening the *abdomen*, I found that the stomach, though it still contained a good deal, was dissolved at its great end, and a considerable part of these its contents lay loose in the general cavity of the belly. This appearance puzzled me very much. The second time was at St. George's Hospital, in a man who died a few hours after receiving a blow on his head, which fractured his skull likewise. From those two cases, among other conjectures about so strange an appearance, I began to suspect that it might be peculiar to cases of fractured skulls; and therefore, whenever I had an opportunity, I examined the stomach in every person who died of that accident: but I found many of them which had not this appearance. Afterwards I met with it in a soldier who had been hanged.

the

the digesting part of the stomach can contain (the shape of the fish-swallowed being very favourable for this enquiry,) we find in many instances that the part of the swallowed fish which is lodged in the digesting part of the stomach is more or less dissolved, while that part which remains in the *oesophagus* is perfectly sound.

And in many of these I found, that this digesting part of the stomach was itself reduced to the same dissolved state as the digested part of the food.

Being employed upon this subject, and therefore enabled to account more readily for appearances which had any connection with it, and observing that the half-dissolved parts of the stomach, &c. were similar to the half-digested food, it immediately struck me that it was from the process of digestion going on after death, that the stomach, being dead, was no longer capable of resisting the powers of that menstruum, which itself had formed for the digestion of its contents; with this idea, I set about making experiments to produce these appearances at pleasure, which would have taught us how long the animal ought to live after feeding, and how long it should remain after death before it is opened; and above all, to find out the method of producing the greatest digestive power in the living stomach: but this pursuit led me into an unbounded field.

These appearances throw considerable light on the principles of digestion; they shew that it is not mechanical power, nor contractions of the stomach, nor heat, but something secreted in the coats of the stomach,

stomach, which is thrown into its cavity, and there animalises the food *, or assimilates it to the nature of the blood. The power of this juice is confined or limited to certain substances, especially of the vegetable and animal kingdoms; and although this menstruum is capable of acting independently of the stomach, yet it is obliged to that *viscus* for its continuance.

* In all the animals, whether carnivorous or not, upon which I made observations or experiments to discover whether or not there was an acid in the stomach, (and I tried this in a great variety,) I constantly found that there was an acid, but not a strong one, in the juices contained in that *viscus* in a natural state.

XXXII. *Experiments and Observations on the Waters of Buxton and Matlock, in Derbyshire, by Thomas Percival, of Manchester, M. D. and F. R. S.*

Read June 25, 1772. **T**HE water of faint Ann's-well is found, by analysis, to contain calcareous earth, fossil-alkali, and sea salts; but in very small proportions: for a gallon of the water, when evaporated, yields only twenty three, or twenty four grains of sediment. It strikes a light green colour with syrup of violets, suffers no change from an infusion of galls, from the fixed vegetable alkali, or from the mineral acids; becomes milky with the volatile alkali, and with Saccharum Saturni; and lets fall a precipitate on the addition of a few drops of a solution of silver, in the nitrous acid. The specific gravity of this water is precisely equal to that of rain water, when their temperatures are the same; but it weighs four grains in a pint lighter, when first taken from the spring. The heat of the bath is about 82 degrees of Fahrenheit's thermometer; that of Saint Ann's well, as it is a smaller body of water, and exposed to the open air, is somewhat less. The water is transparent, sparkling, and highly grateful to the palate *.

* I am indebted to the information of the worthy physician who attends at Buxton, for some of these facts.

In October 1769, I passed a few days at Buxton; and during my stay there amused myself with the following experiments on the effects of the water of Saint Ann's well, on my pulse.

EXPERIMENT I.

October 12, eight o'clock in the morning. The day cold and moist, my pulse beat 84 strokes in a minute; I drank at the well, the third of a pint of water, and, using every necessary precaution, examined my pulse at certain intervals of time; in five minutes, pulse 80, in ten minutes pulse 80, fuller and harder; in twenty minutes pulse 85; in half an hour pulse 90.

EXPERIMENT II.

Eleven o'clock in the forenoon, two hours after breakfast, the air warm and serene, pulse 90; I repeated the draught of water. In seven minutes pulse 109; in fifteen minutes pulse 103; in thirty minutes pulse 100, head-ach; in an hour and a half pulse 95, head-ach abated.

EXPERIMENT III.

October 13, eight in the morning; the day cold, pulse 92; I drank the quantity of water above-mentioned; in five minutes pulse 86; in fifteen minutes pulse 86, full and hard; in twenty minutes pulse 100; in half an hour pulse 92.

From the first and third experiments, it appears that the coldness of the morning counteracted for a time, the effects of the Buxton water; and reduced the
the

the vibrations of my pulse from 84 to 80, and from 92 to 86. But the stimulus of the water soon became superior to the sedative powers of the cold to which I was exposed; for within the space of half an hour my pulse rose to 90 in the first, and to 100 strokes in the second trial. At eleven o'clock before noon, when the air was warm and serene, the water in a much shorter time excited its force, increasing the velocity of my pulse from 90, to 109 vibrations in a minute. These experiments evince the heating quality of Buxton water, and suggest to us the precautions to be observed in the use of it. Small quantities should only be drunk at once, and frequently repeated; the belly should be kept soluble with lenitive Electuary, or any other mild purgative and at the beginning of the course, the patient may be directed to suffer the water to remain a few seconds in the glass, before he swallows it. For this celebrated spring abounds with a mineral spirit, or mephitic air, in which its stimulus, and indeed its efficacy resides, and which is quickly dissipated by exposure to the air.

The honourable and ingenious Mr. Cavendish has shewn by his Experiments on Rathbone Place water, Ph. Transactions, vol. LVII, that calcareous earths may be rendered soluble in water, by furnishing them with more than their natural property of fixed air. And it has lately been discovered that iron also may be suspended by this principle, in the same menstruum *. It appeared therefore highly probable to me, that a chalybeate impregnation might with great facility

* Vid. Mr. Lane's experiments, Ph. Transactions, Vol. LIX.

be communicated to the Buxton water, when fresh drawn from the spring; a quality, which in many cases would add greatly to its medicinal efficacy. I suggested the trial to Mr. Buxton, a very worthy and sensible apothecary near the wells, who has lately at my request made the following experiment.

EXPERIMENT IV.

A quart bottle containing two drachms of iron filings, was filled by immersion, with the water of Saint Anne's well, corked and agitated briskly under the surface of the water: it was then suffered to remain in the well till the filings had subsided, when the water was carefully decanted into a half pint glass; to this were added three drops of the tincture of galls, which immediately occasioned a deep purple colour, and transparency was presently restored by a few drops of the acid of vitriol; evident proofs that a solution of the iron was effected in a few minutes. The water also without the tincture of galls had a chalybeate taste, and left an agreeable astringency on the palate.

By this experiment, it appears that a warm chalybeate abounding with a mineral spirit, and grateful to the taste, may with very little trouble be obtained. And this method of impregnating the Buxton water with iron, must increase its tonic powers, and in many cases improve its medicinal virtues. It is a common practice to join the use of a chalybeate spring in the neighbourhood of St. Anne's well, with that of the Buxton water: but, the superiority of the artificial mineral water must be apparent, if we consider its agreeable warmth, volatility, levity, and gratefulness to the palate.

Buxton bath is very frequently employed as a temperate cold bath. For as the heat of the water is about sixteen or eighteen degrees below that of the human body, a gentle shock is produced on the first immersion, the heart and arteries are made to contract more powerfully, and the whole system is braced and invigorated. But this salutary operation must be greatly diminished, often indeed more than counter balanced, by the relaxing vapours which copiously exhale from the bath, to which the patients are exposed during the time of dressing and undressing. A separate room is indeed provided for the ladies; but the gentlemen have no other accommodations than what the vault affords in which the bath is contained, and are therefore liable to all the inconveniences arising from warmth and moisture. June 12, 1772, the mercury stood in the shade at 65, but in this vault quickly arose to 78 degrees.

EXPERIMENTS ON MATLOCK WATER.

EXPERIMENT I.

A thermometer made by Dollond, and graduated according to Fahrenheit's scale, was exposed for a sufficient length of time, to the steam of the water, as it gushes from the rock, and also immersed in the basin that receives it. The mercury rose to 66 degrees.

EXPERIMENT II.

Six drops of Sp. Sal. Ammon. vol. were poured into a glass of the spring water, which contained
N n n z about

about the sixth of a pint; a very slight cloudiness immediately ensued, but no precipitation was afterwards observable.

EXPERIMENT III.

Six drops of a solution of salt of tartar occasioned a cloudiness, just perceptible, in the same quantity of water; no precipitation ensued.

EXPERIMENT IV.

Six drops of a solution of saccharum saturni immediately produced a milkiness in the water, but no sensible precipitation.

EXPERIMENT V.

Six drops of a solution of silver in the nitrous acid instantly occasioned a milkiness in the water; and after standing an hour, a grey powder was observable at the bottom of the glass.

EXPERIMENT VI.

Ten drops of the infusion of galls neither produced any change of colour in the water at the time they were added, nor was the slightest purple hue perceptible two hours afterwards.

EXPERIMENT VII.

A piece of paper besmeared with syrup of violets was dipped into a glass full of water; no change of colour ensued.

EXPERIMENT VIII.

Another piece of paper, moistened in the same manner with the syrup, was placed over a glass of water, as soon as it was taken from the spring. The paper suffered no change of colour, although it remained an hour upon the glass.

EXPERIMENT IX.

My pulse beat 84 strokes in a minute, at the time when I drank a half pint glass of the Matlock water; in 20 minutes my pulse rose to 86; in half an hour after they sunk to 82, and continued to vibrate the same number of times for an hour, which was as long as I thought it was necessary to examine them.

EXPERIMENT X.

The mercury in the thermometer, when immersed in each of the baths, stood at 68: in the river Derwent, which flows through the valley of Matlock, at 52. These experiments were made in the month of June 1772, and the weather was warm.

EXPERIMENT XI.

A four ounce phial, after being accurately counterpoised in a very nice balance, was filled to the brim with distilled water, which weighed three ounces, four drachms, forty five grains and a half. The same phial, exactly balanced as before, was then filled to the brim with Matlock water, of the same temperature.

perature with the distilled water, which weighed three ounces, four drachms, and forty six grains.

Matlock water is grateful to the palate, and of an agreeable temperature, but exhibits no marks of any mineral spirit, either by its taste, sparkling appearance in the glass, or by the chemical test employed in experiment 8. The second and third experiments shew that it is very slightly impregnated with Selenites or other earthly salts; and of this its comparative levity affords also a further proof: for it weighs twenty-six grains in a pint lighter than the Manchester pump water*, and only four grains heavier than distilled water. The precipitation of a grey powder, by the adding of a solution of silver in aqua fortis to the water, renders it probable that a small portion of sea salt is contained in it. For the powder is found to consist of the particles of silver, combined with the muriatic acid, which is separated from the fossil alkali by the superior affinity the nitrous acid bears to it; and thus a double elective attraction takes place in this experiment.

This water is said to contain iron, but the assertion is at least rendered doubtful by the 6th experiment, which was made with the utmost accuracy; and I am inclined to think, that it is entirely without foundation. The spring is justly celebrated for its efficacy in hæmoptoes; and hence it may have been too hastily concluded that it possesses some slight degree of stypticity, by means of a chalybeate impregnation.

* Vid. the author's treatise on the pump water of Manchester. *Essays medical and experimental*, p. 207. 2d edit.

The 9th experiment, which my short stay at Matlock would not allow me leisure to repeat, affords a presumption that the water is not possessed of any stimulating powers; for the small increase of quickness in my pulse, on drinking half a pint of it, may be ascribed more to the quantity received into the stomach, than to the heating quality of the water,

The Bristol and Matlock waters appear to resemble each other, both in their chemical and medicinal qualities. I have examined and compared them together by the test mentioned above, and so far as such trials may be deemed conclusive, there seems to be no other than the following slight difference between them,

Bristol water becomes a little more milky on the addition of a solution of fixed alkali, and of Saccharum Saturni than that of Matlock; the former also weighs near a grain in a pint heavier than the latter. Is it not to be lamented therefore, that so little attention is paid to Matlock, even by the physicians who reside in the neighbourhood of it? In hectic cases, hæmoptoes, the diabetes, and other disorders, in which the circulation of the blood is rapid and irregular, I should apprehend that Matlock water, on some accounts, claims the preference to that of Bristol; for it is less disposed to quicken the pulse, and may therefore be drunk in larger quantities. But it must be acknowledged that the climate of Bristol is superior to that of Matlock, a circumstance of the highest importance to consumptive patients. Situated in a deep though delightful valley, and surrounded by very high mountains, the sun disappears

at Matlock earlier in the evenings, the fogs are longer in dispersing, and it ~~may~~ be presumed that rain falls here more frequently and copiously than in other places. For at Catworth, which is compassed also with hills, and is about ten miles distant, in 1764, 1765, 1767, and 1768, about 33 inches of rain fell at a medium each year.

The following table exhibits a comparative view of the different temperatures of Bath, Buxton, Bristol, and Matlock waters, measured by Fahrenheit's thermometer.

| * BATH. | |
|------------------|-------|
| King's Bath Pump | 112 ° |
| Hot Bath Pump | 114½ |
| Crofs Bath Pump | 110 |
| * BRISTOL. | |
| Hot Well Pump | 76 |
| BUXTON. | |
| Bath | 82 |
| St. Ann's Well | 81 x |
| MATLOCK. | |
| Baths | 68 |
| Spring | 66 |

* Vid. Mr. Canton's experiments. Ph. Transf. Vol. LVII. p. 203.

XXXIII. *Some Account of a Body lately found in uncommon Preservation, under the Ruins of the Abbey, at St. Edmund's-Bury, Suffolk; with some Reflections upon the Subject: By Charles Collignon, M. D. F. R. S. and Professor of Anatomy at Cambridge.*

Read June 25, 1772. **I**N the month of February last, some workmen, digging among the ruins of the above abbey, discovered a leaden coffin, supposed, from some circumstances, to contain the remains of Thomas Beaufort, Duke of Exeter, uncle to king Henry the Fifth. As it certainly was buried before the dissolution of the abbey, it must have been there between two and three hundred years. It was found near the wall, on the left-hand side of the choir of the chapel of the blessed Virgin; not inclosed in a vault, but covered over with the common earth. Upon examining the appearance of the body, the following circumstances were remarkable, as communicated to me, by an ingenious surgeon, on the spot, Mr. Thomas Cullum.

“ The body was inclosed in a leaden coffin, surrounding it very close, so that you might easily distinguish

guish the head and feet. The corpse was wrapped round with two or three large layers of cere-cloth, so exactly applied to the parts, that the piece, which covered the face, retained the exact impression of the eyes and nose. The dura mater was entire. The brain was of a dark ash colour, with some remaining appearance of the medullary part. The coats of the eye were still whole, and had not totally lost their glistening appearance. There was about half a pint of a bloody-black water in the thorax; and a mass that seemed to be part of the lungs. The pericardium and diaphragm were quite entire. The abdominal viscera had been taken out very clean, and the integuments and muscles stuck very close to the vertebrae of the back. This cavity looked fresher than that of the thorax. I cut into the psoas magnus, where there were evident marks of red muscular fibres. The other muscles had lost all their red colour, and were become of a dark brown. The tendons were still strong, and retained their natural appearance. The hands, which are preserved in spirits, retain the nails. There were some very small holes in the coffin, out of which had run some bloody water, of an offensive smell. All the principal blood-vessels must have been cut through, in taking out the abdominal viscera: and if no ligature was made upon the vessels, their contents would escape, particularly as assisted by the pressure of the cere-cloth, which is of considerable weight, and, doubtless, put on hot. This fluid running out of the coffin, upon its being moved, might occasion the suspicion of the body being put in pickle."

Thus

Thus far Mr. Cullum's account, by which it appears, that the viscera of the abdomen had been taken out, so that the greatest part of the blood, he observes, did probably flow out, during that operation, from the mouths of the divided vessels, and whose diameter is considerable. This would greatly reduce the quantity of the fluids. The holes in the coffin, if purposely made, would seem designed to let out extravasated or transfusing fluids; but are irreconcilable with the notion of the body being in pickle. If the holes were accidental, the notion of a pickle may still be allowed. Might not the cere-cloth, impregnated, perhaps, with gums or resins, and, from its taking so exact an impression, most probably laid on hot preclude the external air; and, if done immediately after the party's death, obviate the deposition of eggs, or incapacitate them from ever hatching? The lead grasping close, would co-operate with the cere-cloth in the exclusion of air and insects.

We have undoubted accounts of bodies found very little changed, after long interment, where there was no appearance of any art having been used. And there is no doubt some constitutions are more prone to putrefaction after death than others; these circumstances may be dependant on the age, sex, and last disease; to which predisposing causes, thus attending persons to the grave, are to be added the soil and situation in which they are deposited. Could we be masters of all these particulars, in the few dead bodies hitherto discovered greatly free from the usual putrefaction, it would lead, perhaps, to the probable

cause of the phenomenon, and point out a proper method of imitation. And till that is done, it is difficult to know how much merit is to be assigned to the art or mystery of embalming, and how much to the power of natural causes.

XXXIV. *A Letter from Richard Pulteney, M. D. F. R. S. to William Watson, M. D. F. R. S. concerning the medicinal Effects of a poisonous Plant exhibited instead of the Water Parsnep.*

DEAR SIR,

Read July 9, 1772. **S**OME circumstances having lately come to my knowledge, relating to the effects of a poisonous plant, I thought them rather too remarkable not to merit further notice; and, I address them to you with the more propriety, as you have already laid before the publick some observations * concerning the deleterious qualities of the plant in question, which holds a distinguished place among the poisonous ones that are indigenous in Britain.

Mr. H——n, an attorney of this place, now upwards of forty, at the age of fifteen, began to be affected (after taking cold upon violent exercise, as he thinks) with what is usually called a scorbutick disorder; which shewed itself more particularly on the outsides of his arms, about the elbows, and on

* See Philosophical Transactions, Vol. XLIV. p. 227. and Vol. L. p. 856.

the outfides of his legs, from the knees to the ancles, as well as in blotches upon other parts of his body. It had the appearance of a dry branny scab or scurf, which every night fell off, more or lefs, in fcales, as is ufual in leprous cafes. At times it pushed out more than ufual, and thickened the integuments of the limbs confiderably, after which the feparation of fcales would become very abundant.

For feveral years paff he had been trying a variety of things commonly recommended in fuch cafes, particularly the quack medicine known by the name of Marcdant's Drops, which he continued for near a twelvemonth, without finding the leaft fenfible relief: alfo an ele&tuary of Flos fulphuris and Cremor tartari, which he had persevered in for near three years, without finding any other alteration, than that of its preventing coftivenefs, to which he was habitually fubject.

In the winter 1770, this diforder increafed upon him very rapidly, without being able to affign any reafon, from any accident that had happened to him, or from any irregularity of his own in point of regimen, in which he was always very exact. At this time, befides the farther fpredding of the eruption itfelf, the integuments of the legs thickened very much, and the limbs fwelled to fuch a degree, as to render him unable to walk. The quantity of branny fcurf and fcales thrown off, at this time, was very great; he fays "handfuls might have been taken out of his bed every morning."

In this unhappy fituation, even loathfome to himfelf, it was recommended to him to take the juice of water parfnep, in the quantity of one common table-
 fpoonful

spoonful every morning, fasting, mixed with two spoonfuls of white mountain wine.

Accordingly, about the middle of January 1771, he procured a half-pint phial of what was so called, by means of the person who had recommended it, and who had assured him that he had been greatly relieved, in a similar disorder, by it.

The first spoonful he took did not begin to give any great uneasiness for two hours, but after that time, his head began to be affected in a very extraordinary manner; a violent sickness soon succeeded, and violent vomiting; and, after he was put to bed, there came on cold sweats, and a very strong and long-continued rigor, so that the people about him thought him dying for some time; but, in a few hours, all these symptoms wore off.

Such, however, had been the inveteracy of his disorder, and so strong his desire to find relief, that he determined not to desist; and, after having omitted his medicine for one day, he repeated it, in nearly the same dose, and with similar effects as to sickness and vomiting, though the uncommon sensation in his head, and the succeeding rigor, were by no means so violent. He had resolution enough to continue this dose every other morning, for more than a fortnight, and then reduced it to three teaspoonfulls which was just the half of the first dose.

Before he had taken this juice one month, he was sensible of a very great change for the better; encouraged, therefore, by these appearances he persevered in its use until the middle of April, by which time his skin, though not quite cleared, yet had ceased to throw off any more scurf, was be-
come

come soft, clean, and well conditioned, and, as he has repeatedly assured me, he got then into a much better conditioned state, than he had experienced for many years before.

From first to last, this juice never purged him; though he says, even in its reduced dose, it never failed to occasion a dizziness of the head, a nausea, and sickness, which were not infrequently succeeded by a vomiting, that always instantly relieved his head.

From the middle of April to the middle of June, he desisted from the use of the juice, but, in its stead, drank every morning for breakfast, the infusion of the leaves of the same plant, which, he says, is like common bohea tea. The infusion seldom occasioned nausea, or sickness, but always brought on a small degree of vertigo, and in a slight manner produced the effects of intoxication from liquor.

In June he went to Harrowgate, as he had designed in the summer before. Upon first drinking and bathing there, he thought himself worse; and his eruptions, having gradually increased during the two months that he staid in that place, he was convinced that those waters were of no real service to him. On his coming home, he returned to the use of the infusion, and he assures me, that he again found, even by that weak preparation, a very speedy alteration for the better. From that time, he continued it ever since, until his stock of the herb was exhausted; his skin is now so very little affected, that he has but here and there, upon his arms and legs, a very small appearance of his disorder.

Upon questioning him relating to the sensible qualities of this medicine, he says again, that he
part-

particularly remembers that it never once purged him; not even the first dose, which had so nearly poisoned him. He does not think that it increased the sensible perspiration, but is convinced that it was diuretick; and adds, that he thinks it occasioned, besides the increased flow of urine, a copious sediment in it, and which he believes was always wanting before.

This is the plain, narrative of the fact. He has assured me that no medicine or regimen, among the great variety that he has tried, ever had any sensible effect upon his disorder before; and that nothing but the very early and sensible relief he experienced from this juice, could have induced him to persevere in its use, under such uneasy feelings, as it never failed to produce. Indeed, he makes nothing of the lighter effects of the infusion, from which, however, he thinks, he has likewise reaped no small benefit. ❀

This case, the nature and inveteracy of his disorder, being well known among his neighbours, was much talked of, and raised the curiosity of many people. When I first heard of it, and was informed of the smallness of the dose, and its virulent operation, I could scarce doubt that the juice of some other plant had been administered instead of that of the water parsnep, which we know to be a safe and harmless vegetable; medical writers having directed its juice to be drunk, even to the quantity of four ounces for a dose: and as I know, the *Oenanthe crocata*, hemlock dropwort, to be exceedingly plentiful in this country, so much, as to be more easily procured than the water parsnep itself; I thought it

probable that that plant had been used in its stead. Upon getting a specimen, it appeared that this had been indeed the case; as also, upon further enquiry, that it was the juice of the root only, and not of the leaves and stalks, that had been administered. I might here observe, that the expression from the root is not to be depended upon after the plant is advanced towards its flowering state, as the root then becomes light, spongy, and almost destitute of juice.

If you judge this case not improper to be laid before the Royal Society, you will do me the honour of presenting it. Mr. H——n himself is so much convinced of the efficacy of the medicine, that he is desirous of its being known to the world.

I do not enter into any reasoning on this occurrence; I relate it only as a fact, and desire it may have no more weight than every judicious physician knows is due to a single instance. How far it may be proper to give this juice a farther trial, I will not take upon me to determine; but must, as an encouragement to any who may chuse to venture upon it, inform them, that it has not on all persons so much power in producing nausea and sickness, as in the case here before us. I am,

S I R,

with great esteem,

Your obliged humble servant,

Blandford,
March 12, 1772.

R. Pulteney.

P. S.

P. S. Mr II—— is desirous that it should be known, that he “ tried very fruitlessly, among other methods, the drinking of tar-water and sea-water, of each of which, he says, he did not drink less than an hoghead.”

XXXV. April 21, 1772. *Experiments on two Dipping-Needles, which Dipping-Needles were made agreeable to a Plan of the Reverend Mr. Mitchell, F. R. S. Rector of Thornhill in Yorkshire, and executed for the Board of Longitude, by Mr. Edward Nairne, of Cornhill, London.*

Read July 9,
1772.

THE magnetic needles were twelve inches long, and their axes (the ends of which were of gold allayed with copper) rested on friction-wheels of four inches diameter, each end on two friction-wheels, which wheels were balanced with great care. The ends of the axes of the friction-wheels were likewise of gold allayed with copper, and moved in small holes made in bell-metal; and opposite the ends of the axes of the needles, and the friction-wheels, were flat agates, finely polished. Each magnetic needle vibrated in a circle of bell-metal, divided into degrees and half-degrees, and a line passing through the middle of the needle to the ends pointed to the divisions. The minutes set down in the experiments were, by estimation, as the third of half a degree is counted ten minutes. The instruments were carefully placed, so that the needles vibrated exactly in the magnetic meridian.

meridian. The two needles were nearly balanced before they were made magnetical; but, by a curious contrivance of the Reverend Mr. Mitchell of a cross fixed on the axes of the needles (on the arms of which were cut very fine screws, to receive small buttons, that might be screwed nearer or farther from the axis), the needles could be adjusted both ways, to a great nicety, after they were made magnetical, by reversing the poles, and changing the sides of the needle.

First set of experiments made by Edward Nairne, at his house, N^o 20, Cornhill.

72 20
72 20
72 20
72 20
72 20
72 20.

Second set of experiments, with that side of the instrument to the East, which was to the West in the first observation.

72 10
72 15
72 45 } Here the ends of the axis touched the
72 45 } agates.
72 5
~~72.~~

Third

Third set of experiments, in which the poles of the needle were reversed, but the same side of the instrument to the East, as in the second set of experiments, and the needle rather more magnetical, being touched with a larger set of magnets.

72 30
 72 30
 72 30
 72 30
 72 30
 72 30.

Fourth set of experiments, viz. the same side of the instrument to the East, as in the first set of experiments.

72 10
 72 10
 72 15 Observed by Mr. Wales.
 72 10
 72 10
 72 10.

Fifth experiment, viz. the same end of the needle made North, as in the first set of experiments, and also the same side of the instrument to the West, as in the first set of experiments.

72 20.

Experiments

Experiments made April 22, 1772, with the other Dipping-needle, the instrument being put in the same place, and with great care, in the magnetic meridian, the needle pointed as under.

72 15

72 10 The poles of the needle changed.

72 20 { The side of the instrument to the East, which in the first observation was to the West.

Left any thing magnetical should have affected the needle in Mr. Nairne's house, he took this instrument, and placed it in the middle of a large room belonging to the London Assurance in Birchin-Lane, and then the needle pointed to

72 10 or 15

72 20

72 30 The poles of the needle changed.

72 10 { The side of the instrument to the East, which in the first observation was to the West.

The dipping-needle brought back to Mr. Edward Nairne's, and put in the same place as before, stood at

72 10 +

The

In the foregoing experiments, the needle was raised to an horizontal position, and left to vibrate. It was between 8 or 9 minutes before the vibration ceased.

The needle brought to an horizontal position, and one grain and a half laid on the extremity of the South end, was not sufficient to keep it in an horizontal position; but the North end pointed to $35^{\circ} 30'$. One grain and three quarters laid on the extremity of the South end of the needle, was more than sufficient to keep it in an horizontal position, the South end then pointing $6^{\circ} 45'$ below 0.

It having been judged proper to have a Drawing of the Dipping-Needle, the following Plate [TAB. XIII.] has been made, wherein

- AA Represents the needle.
- BB The ends of the axis resting on the friction-wheels.
- CCCC The four friction-wheels.
- DDD Where flatagate caps are set in.
- EEE The divided circle of bell-metal.
- FFFF The ends of the cross for adjusting the needle.
- GG Two levels, whereby the line of 0 degrees of the instrument is set horizontal.
- H The perpendicular axis, whereby the instrument may be turned, that the divided face of the circle may front the East or West.
- I An index fixed to the perpendicular axis H, and which points to an opposite line on the horizontal plate K, when the instrument is turned half round.
- LLLL Four adjusting screws to set the instrument horizontal. One of them is hid behind the circle.
- MMMM Screws which hold on the glass covers, to keep the needle from being disturbed by the wind.

A N
I N D E X
T O T H E
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 144. l. 2, r. Dena. *Notes, l. 14. from the bottom, r. Noewera,*
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